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ABSTRACTS

OF

THE PAPERS PRINTED IN

THE PHILOSOPHICAL TRANSACTIONS.

The Croonian Lecture. On the Structure and Uses of the Membrana Tympani of the Ear. By Everard Home, *Esq. F.R.S.* Read Nov. 7, 1799. [*Phil. Trans.* 1800, *p.* 1.]

THE part of the ear which was the subject of the present lecture, has always been considered as a common membrane, which, being stretched or relaxed by means of muscles belonging to the malleus, became fitted in its various degrees of tension to convey the immense variety of external sounds to the internal organ.

Though this description have been generally adopted, yet it will appear upon further inquiry that, owing no doubt in a great measure to the extreme minuteness of the part, the structure and some of the properties of this membrane had not as yet been properly investigated. And the discovery here announced is brought forward with the greater propriety on this occasion, as it affords a new instance of the application of muscular action, which may ultimately account for certain phænomena in the sense of hearing in a more satisfactory manner than has hitherto been proposed.

This discovery we owe in some measure to the opportunity Mr. Home had to dissect the ear of an elephant, where the parts are so much larger even than they ought to be in proportion to the size of the animal, that the structure of this membrane, which is usually denominated the Drum of the Ear, becomes obvious even to the naked eye. On close examination it was found, that instead of being an uniform coat or skin, a great number of muscular fibres, which seem incorporated in it, pass along its texture in a radiated manner from the rim which surrounds it, towards the handle of the malleus, with which the central part of the membrane is firmly connected.

Having thus been put upon the scent, the membrane in the human ear was carefully separated from its contiguous parts, and being viewed in a microscope, magnifying 23 times, exhibited the same

texture, the muscular fibres appearing similar throughout the whole surface; without any central tendons as in the diaphragm, and chiefly forming the internal layer, on which side they appeared most conspicuous.

The blood-vessels of this membrane, the number of which is proportionate to its action, resemble in a great measure those of the iris, and are nearly as numerous. They anastomose with one another in a similar manner, and their general dissection is from the circumference of the membrane to the handle of the malleus. From near this handle a small trunk sends off branches in a radiated manner, which likewise anastomose with those that have an opposite course. This correspondence in the number and distribution of blood-vessels, between the membrana tympani and the iris, is given as a proof of that membrane being endowed with muscular action; and indeed the author henceforth speaks without hesitation of a radiated muscle distinct from the membrane. Having examined the structure of this muscle in various animals, Mr. Home found the application of a principle laid down in a former lecture, namely, that while the organization necessary for muscular contraction may in some measure exist in an apparent membrane, yet wherever muscular action is to overcome a resistance, a fasciculated structure becomes absolutely necessary. Thus in birds, where from the smallness of the organ the resistance is very trifling, this membrane is of a simple structure, like the coat of the hydatid; whereas in the elephant the fibres forming fasciculi are very distinct and obvious.

This muscular structure in the membrana tympani serves to explain various phænomena in hearing, which were not hitherto clearly understood. It is observed that the sounds produced by percussion on an extended membrane like the drum and tambourine, cannot but be imperfect; since, the tension being from side to side, the instrument may be considered as consisting of a number of parallel strings varying in their lengths like the parallel chords of a circle, and bearing consequently no musical proportion to each other. Such also must have been the imperfection of the membrana tympani, according to the received opinion of its structure: but after the present discovery it will appear obvious that the radiated muscle proceeding from the circumference of the circle to a common centre, produces a system of strings all equal in length, and uniformly varying in musical effect, according to their tension.

In order to illustrate the manner in which this radiated muscle adapts the membrana tympani to different sounds, the author finds it necessary to enumerate the more important parts of the organ, and to point out the use commonly assigned to each of them.

Without entering into this detail, we can here only observe that, availing himself of the present discovery, he compares the tensor and radiated muscles of the membrana tympani to a monochord, of which the membrane is the string, the tensor muscle the screw giving the necessary tension to make the string perform its proper scale of vibrations, and the radiated muscle acting upon the membrane like the

moveable bridge of the monochord, adjusting it to the vibrations required to be produced. Thus the combined effects of the action of these muscles give the perceptions of grave and acute tones; and in proportion as their original conformation is more or less perfect, so will their action be, and consequently the perceptions of sound which they communicate.

This mode by which the membrana tympani is capable of being adjusted to certain tones, or rather musical keys, will it is thought fully account for the difference between a musical ear, and one which is too imperfect to discriminate different notes with any degree of nicety. This delicacy of the ear, as it is found to depend on muscular action, may therefore be in some measure acquired, and is likewise liable to be impaired by illness or other accidental causes, of which some striking instances are here related.

In endeavouring to explain the uses of the more internal parts of the ear, considerable advantage, it is thought, may be derived from classing them in two divisions, namely, those which are formed for the purpose of receiving impressions conveyed through the medium of liquids or of solid substances; and those adapted to receive impressions made by the impulses of an elastic fluid such as common air. The former are the ears of fish, which are found to have fewer parts than those of birds, quadrupeds and man; but in the latter we find that the organ is susceptible of impressions by both vehicles. Thus men can hear the ticking of a watch by applying it to the forehead, and shutting the ears: the sound in this instance being evidently conducted through the bones of the skull, it appears manifest that only the interior parts of the ear, namely, the vestibulum and semicircular canals, co-operate to produce this sensation; and these in fact are the principal parts of that organ in fish.

In birds the membrana tympani has no tensor muscle to vary its adjustment, and hence their scale of sounds cannot descend so low as in the human ear. The cochlea, which has hitherto been considered as the part of the organ by which sounds are modulated, is also wanting in birds, which, however, are known to have a singular nicety in discriminating inarticulate sounds; and hence, as well as on account of its being filled with water instead of air, which renders it less capable of modifying sounds, it is manifest that this is not the real use for which the cochlea is destined. What is its precise use, as well as of the semicircular canals, remains yet to be investigated.

Lastly, it is observed that in the elephant there is no bony septum separating the cells of the skull belonging to one ear from those which open into the other, but a free communication exists between them: from this, the enlarged proportions of the organ and some other circumstances here mentioned, it is inferred that the sense of hearing must be quicker in this than in any other animal. And in fact some curious instances are mentioned which seem fully to confirm this assertion.

On the Method of determining, from the real Probabilities of Life, the Values of Contingent Reversions in which three Lives are involved in the Survivorship. By William Morgan, *Esq. F.R.S.* Read Dec. 12, 1799. [*Phil. Trans.* 1800, *p.* 22.]

Mr. Morgan having already communicated to the Society the solutions of seventeen different problems in the doctrine of contingent reversions depending upon three lives, has been induced, from a wish to complete the subject, to investigate in the present paper seven more problems in which the same number of lives are concerned in the survivorship. These, he tells us, include, as far as he can perceive, all the remaining cases involving those complicated contingencies.

In examining the investigation of these problems, it appears that the determination of the reversion in some of them depends in each year on the happening of twelve or thirteen different events. These numerous contingencies being all expressed by separate fractions (each of which is resolved into two or more different series) renders the operations exceedingly intricate and laborious. From an apprehension, it seems, of becoming tedious and diffusive in his demonstrations, the author has in general contented himself with merely giving the fractions denoting the contingencies on which the reversion depends, without specifying in words at length the nature of those contingencies. He has, however, in these as in all the other problems he has investigated, given different demonstrations, both by solving each independent of any other problem, and by deriving the solution from those of two or more problems, which had been already investigated; so that from the exact agreement in the results, proofs are deduced of the perfect accuracy of the demonstration, not only of the problem investigated, but also of those which are applied to the solution.

In all these problems, a contingency is involved, which having never been accurately determined, had hitherto rendered even an approximation to the solution of them impossible. This contingency is that of *one life's failing* after another in a given time. This appears to have been ascertained with sufficient accuracy to enable the author to surmount a difficulty in the solution of these problems, which he owns he had once considered as insuperable.

Having thus accomplished the investigation of every case in which he conceives it possible that the contingency may be varied between these lives, he conceives that he has now exhausted the subject; and concludes his paper with observing, that those cases in which four lives are involved in the survivorship are not only too numerous and complicated to admit of solution, but that they occur so seldom in practice as to render the labour of such solution (if it were practicable) both useless and unnecessary.

Abstract of a Register of the Barometer, Thermometer, and Rain, at Lyndon, *in* Rutland, *for the year* 1798. *By* Thomas Barker, *Esq.* Read Dec. 12, 1799. [*Phil. Trans.* 1800, *p.* 46.]

On the Power of penetrating into Space by Telescopes; with a comparative Determination of the Extent of that Power in natural Vision, and in Telescopes of various Sizes and Constructions; illustrated by select Observations. By William Herschel, *LL.D. F.R.S.* Read Nov. 21, 1799. [*Phil. Trans.* 1800, *p.* 49.]

It has long been observed that the power of distinguishing objects at great distances depends not only on the magnifying power applied to the telescope through which they are viewed, but also on the quantity of light emitted by the object, and collected and conveyed to the eye by means of the instrument. The superiority of telescopes with large apertures must hence appear obvious; and we have long witnessed the essential improvements made in this respect by Dr. Herschel, which have enabled him to extend his view into the firmament to distances, the bare mention of which is sufficient to astonish a mind unaccustomed to investigations of this nature. That it is principally the increased quantity of light that enables us to view luminous objects at great distances will appear manifest if we reflect that, since the density of light decreases in the ratio of the squares of the distances of the objects emitting the light, it follows that an object may be removed to distances at which its light will be so rarefied as to produce no longer any sensation upon the optic nerve: that if an optical instrument be used with an object-glass of a larger diameter than the pupil of the eye, the quantity of light collected by this means in the eye will be greater in proportion to the greater extent of the object-glass compared with that of the pupil: and that hence the most distant star that can be seen with the naked eye, if it be viewed through a tube with an object-glass of twice the diameter of the pupil, it will without any magnifying power be visible at a distance four times greater than that at which the naked eye ceased to perceive it. Dr. Herschel many years ago adverted to this circumstance, when in his paper on the Construction of the Heavens, he introduced what he then figuratively called *his sounding line*, to which he now substitutes the appellation of the *power of penetrating into space*. And in the present paper he fully investigates a comparative determination of the extent of that power in natural vision, and in telescopes of various sizes and constructions; all which he illustrates by a number of select and curious observations.

In the first part of the paper he establishes the difference between magnifying and penetrating powers; he rejects some vague terms in common acceptation, to which he substitutes algebraic symbols and such accurate definitions as enable him to proceed upon solid ground. And after distinguishing between self-luminous objects and those which shine by a reflected light, and likewise noticing those whose

brightness is the effect of a considerable depth of luminous matter, he shows that these differences noways affect the present inquiry; since in all these several bodies, it is the quantity of light emitted by their surfaces which becomes the object of our perception. As the same body, however, may be differently luminous in different parts of its surface, he exhibits a formula by which the aggregate brightness of a body may be estimated. And he closes this part with an examination of the opinion maintained by Lambert in his Système du Monde, where he says that an object is equally bright at all distances, and that the sun at the distance of Saturn, or still further from us, would be as bright as it is in its present situation. This assertion taken in the general sense in which it is here expressed, he proves to be a palpable contradiction; and only admits it in as far as the celebrated author may mean the *intrinsic brightness* of a body, which applies to its surface diminished by distance, and not the *absolute* brightness of the whole.

In the next section the author endeavours to ascertain the general extent of vision with the naked eye. As to those bodies which shine with a reflected light, he asserts that none have yet been seen more distant than the Georgian planet: admitting this as the maximum, it must after all excite our admiration that borrowed light should be perceptible to our naked eye at the distance of no less than eighteen hundred millions of miles; especially if we consider that the light of the sun on that planet is above 368 times less intense than it is on our earth, and that probably two thirds of that diminished light is absorbed by the planet.

The range of natural vision, with respect to self-luminous objects, is incomparably more extended. Passing over the intermediate steps by which our author arrives at his conclusions, we shall simply mention his inference that no single star above nine or at most ten times more distant than Sirius can possibly be perceived by the naked eye; admitting, however, that an accumulation of stars will be perceptible at a far greater distance.

From the power of penetrating into space by naked vision, our author proceeds to that of telescopes. Here he first calculates, by a method recommended by Mr. Bouguer, the quantity of light absorbed and dissipated by the reflection of the mirror, and refraction of the eye-glasses; and he finds that a common Newtonian with three lenses loses about $\frac{6}{10}$ths of the whole light it receives, and that in a telescope of his own construction with two lenses this loss amounts to somewhat less than $\frac{4}{10}$ths. The Doctor now enters into a full investigation of the penetrating power of his several telescopes under all the various circumstances he could devise, and illustrates the whole by a great number of observations, which serve to confirm the inferences deduced by him. Here we learn that the penetrating power of his 20-feet reflector, applied to a single star, may extend as far as 612 times the distance of Sirius, and also that his large telescope, with a penetrating power of 192, will show a single star of the 1342nd magnitude.

In the next sections he shows that the penetrating and magnifying powers, so far from assisting each other, will often prove reciprocally detrimental, which he thinks may be explained by admitting that while the light collected is employed in magnifying an object, it cannot be exerted in giving penetrating power, to which perhaps ought to be added the detrimental effect of the magnifying power on the heterogeneous ingredients floating in the atmosphere. Whatever be the cause, the fact is proved by various observations.

Lastly, he shows that as we must not limit our vision within the sphere of the single stars, we must call the united lustre of the sidereal system to our aid in stretching forward into space. Supposing one of these clusters of 5000 stars to be at one of those immense distances to which only a 40-feet reflector can reach, he calculates that this distance will exceed at least 300,000 times that of the most remote fixed star visible to the naked eye. He concludes with a rough calculation how much time it would take to sweep the heavens with a penetrating power of such an immense extent; and finds that in this climate, with his 40-feet reflector, with a magnifying power of 1000, this operation for the whole sphere would take no less than 811 years.

A second Appendix to the improved Solution of a Problem in physical Astronomy, inserted in the Philosophical Transactions for the Year 1798, *containing some further Remarks, and improved Formulæ for computing the Coefficients* A *and* B; *by which the arithmetical Work is considerably shortened and facilitated. By the Rev.* John Hellins, *B.D.F.R.S. and Vicar of* Potter's Pury *in* Northamptonshire. Read Dec. 12, 1799. [*Phil. Trans.* 1800, *p.* 86.]

This paper relates to an improved solution of a problem by which swiftly converging series are obtained, which are useful in computing the mutual perturbations of the planets; and contains some further remarks and improved formulæ for computing the coefficients, by which the arithmetical work is considerably shortened and facilitated.

Account of a Peculiarity in the Distribution of the Arteries sent to the Limbs of slow-moving Animals; together with some other similar Facts. In a Letter from Mr. Anthony Carlisle, *Surgeon, to* John Symmons, *Esq. F.R.S.* Read Jan. 9, 1800. [*Phil. Trans.* 1800, *p.* 98.]

This peculiarity was first observed in the axillary arteries and in the iliacs of the Lemur tardigradus, which at their entrance into the upper and lower limbs were found to be suddenly divided into a considerable number of equal-sized cylinders, which occasionally anastomosed with each other, and were regularly distributed on the muscles; whilst the arteries proceeding to the other parts of the body divided in the usual arborescent form.

Upon prosecuting this inquiry, it was found that the Bradypus

tridactylus, and in some measure also the didactylus, has a similar distribution of these arteries.

This peculiar disposition of the arteries in the limbs of these slow-moving quadrupeds, it is thought cannot but retard the velocity of the blood passing into the muscles of the limbs. Whence the well known sluggishness of the animals, to whom this configuration seems as yet peculiar, will perhaps be ultimately accounted for. Something similar has been observed in the carotid artery of the lion, which it is thought may be subservient to the long continued exertion of the muscles of his jaws, in holding a powerful animal for a length of time; and lastly, it is conjectured that the ruminating animals have a somewhat similar aplexus of arteries in the neck, which operates in retarding the velocity of the fluids in those parts.

Outlines of Experiments and Inquiries respecting Sound and Light. By Thomas Young, *M.D. F.R.S. In a Letter to* Edward Whitaker Gray, *M.D. Sec.R.S.* Read Jan. 16, 1800. [*Phil. Trans.* 1800, *p.* 106.]

We are here presented with a numerous set of experiments and observations, which the author does not deliver as a series calculated to elucidate any particular object, but rather as the results of the first steps of an investigation; which being of considerable magnitude, and not to be accomplished in a short period of time, are here brought forward in a detached form, in order to preserve them from oblivion, should any unforeseen circumstances prevent his continuing the pursuit. They are classed under sixteen different heads, of which the following are the titles, and some of the principal inductions.

1. *Of the Quantity of Air discharged through an Aperture.*—This was deduced from the quantity of pressure of water, on a body of air rushing through a small aperture at the end of a tube. The result was, that the quantity of air discharged by a given aperture is nearly in the subduplicate ratio of the pressure; and that the ratio of the expenditures by different apertures, with the same pressure, lies between the ratio of their diameters, and that of their areas.

2. *Of the Direction and Velocity of a Stream of Air.*—These were determined by the stream, produced by a known pressure, being made to impinge, in a perpendicular direction, against a white plate, on which a scale of equal parts was delineated, and which was thinly covered with a coloured liquid. The results were here inferred from the breadth of the surface of the plate laid bare by the stream.—The experiments being repeated at different distances between the orifice and the plate, the longitudinal form of the stream could be hence deduced, their sections being bounded by curves, the nature of which could be determined by their ordinates and abscissæ. The numerous results obtained in this manner are entered in various tables, and likewise illustrated by figures, in which the longitudinal and not the transverse sections are exhibited to the eye.

3. *Ocular Evidence of the Nature of Sound.*—This is produced by

a stream of air driven through a pipe with a lateral aperture, like a French flute, where at the part where the stream issues out of the orifice, vibrations are manifestly perceived, which are rendered still more evident if the current be impregnated with smoke.

4. *Of the Velocity of Sound.*—The velocity of any impression transmitted by the common air, being corrected by the experiments of various observers, is at an average here estimated at 1130 feet in a second of time.

5. *Of Sonorous Cavities.*—What is here said relates chiefly to the reflection of sounds in rooms or galleries. This we find takes place as often in a second as double the breadth of the room or passage is contained in 1130 feet, that breadth determining the pitch of the musical note thereby produced.

6 *Of the Divergence of Sound.*—Various observations are here related which militate against the received opinion that sound diverges equally in all directions, and that there is no substance impervious to sound. On this head, however, the author admits that a more ample investigation will be required than has hitherto been instituted; and he intimates that he shall engage in it as soon as his leisure will permit.

7. *Of the Decay of Sound.*—The two hypotheses, 1st, that sound decays nearly in the simple ratio of the distances, and 2nd, that this diminution is in the subduplicate ratio, are here stated, and some fallacies are mentioned, which will likewise render a further inquiry necessary.

8. *Of the Harmonic Sounds of Pipes.*—The object of this section appears from a table exhibiting the results of a set of experiments, made with a view to ascertain the velocity with which organ-pipes of different lengths require to be supplied with air, according to the various appropriate sounds which they produce.

These were made on pipes of the same bore, and of different lengths, both stopped and open. The general result was, that a similar blast produced as nearly the same sound as the length of the pipe would permit, or at least that the exceptions, though very numerous, lie equally on each side of this conclusion.

9. *Of the Vibrations of different Elastic Fluids.*—The difference of these vibrations has been received as being reciprocally in the subduplicate ratio of the density of the fluid. Hence in pure hydrogen gas they must be 3·6 times greater than in common air. And this will explain why an instrument will often appear out of tune, when, in fact, the fault lies in the change of temperature of the atmosphere; and why the pitch of an organ will be found to differ materially in summer and winter.

10. *Of the Analogy between Light and Sound.*—While the author vindicates the Newtonian theory of light against the criticisms of Euler, he freely admits that it is liable to some objections, among which he chiefly insists upon the uniformity of the motion of light in the same medium, and the partial reflection from every refracting surface. Having reasoned largely upon this subject, he admits the

probability of an analogy between the colours of a thin plate and the sounds of a series of organ-pipes; and observes that the same colour recurs whenever the thickness of the plate answers to the terms of an arithmetical progression, in the same manner as the same sound is produced by means of an uniform blast from organ-pipes which are different multiples of the same length.

11. *Of the Coalescence of Musical Sounds.*—In this section Dr. Smith's assertion that the vibrations constituting different sounds are able to cross each other in all directions, without affecting the same individual particles of air by their joint forces, is minutely investigated and controverted.

12. *Of the Frequency of Vibrations constituting a given Note.*

13. *Of the Vibrations of Chords.*

14. *Of the Vibrations of Rods and Plates.*

Of the contents of these three sections no account will be here attempted, as they consist chiefly of experiments and demonstrations illustrated by diagrams.

15. *Of the Human Voice.*—A technical description is here given of the formation of sounds by the configuration and inflexions of the different parts of the vocal organ. And it is intimated that by a close attention to the harmonics entering into the constitution of various sounds, much more may be done in their analysis than could otherwise be expected.

16. *Of the Temperament of Musical Intervals.*—After pointing out some imperfections in most of those who have treated this subject before him, the author suggests his own method of distributing the imperfection of the scale, so as to produce a modulation that shall be found the least defective. And here he observes, as upon an average of all music ever composed some particular keys occur at least twice as often as others, there seems to be a very strong reason for making the harmony the most perfect in those keys which are the most frequently used; since the aggregate sum of all the imperfections which occur in playing, must by this means be diminished the most possible, and the diversity of the character at the same time accurately preserved.

Observations on the Effects which take place from the Destruction of the Membrana Tympani of the Ear. By Mr. Astley Cooper. *In a Letter to* Everard Home, *Esq. F.R.S., by whom some Remarks are added.* Read Feb. 6, 1800. [*Phil. Trans.* 1800, *p.* 151.]

The case to which we owe the observations contained in this paper, is that of a youth, who at the age of ten was attacked with an inflammation and suppuration in the left ear, which continued discharging matter for several weeks; and who after the space of about a twelvemonth had the same symptoms in his right ear, the discharge in both cases being thin and fetid, and conveying many small bones and particles of bones. On probing the ears when the youth was about twenty years of age, it was found that the membrana tympani

of the left ear was totally destroyed, and that but a small part of it remained in the right ear. So free was the passage through both the ears, that the patient, by closing his nostrils and contracting his cheeks, could with ease force the air from the mouth through the meatus auditorius; and yet what is most remarkable, the sense of hearing was by no means materially impaired by this imperfection; especially in the left ear, where the whole of the membrane was dissolved. The organ even retained a nice musical discrimination.

From this, and another similar instance here described, it is inferred that the loss of the membrana tympani, though it somewhat diminishes the power of hearing, does not absolutely destroy it; and that probably where this membrane is wanting, its functions are supplied by the membranes of the fenestræ ovalis and rotunda, which being placed over the water of the labyrinth, will, when agitated by the impressions of sound, convey their vibrations to that fluid, in a similar manner as is done by the membrana tympani in its healthy state. It is also intimated that the principal use of this membrane is probably to moderate the impressions of sound, and to proportion them to the powers and modifications of the organ.

Mr. Home, by whom this paper was communicated, has been pleased to add some additional remarks on the mode of hearing in cases where the membrana tympani has been destroyed; from which we collect that this membrane appears to him to be chiefly intended to give an extended surface capable of communicating to the small bones the impressions made upon it, which a membrane would be incapable of doing unless it had a power of varying its tension to adapt it to different vibrations: and that this membrane being destroyed, there can be little doubt that the impressions of the vibrating air are produced on the stapes, from whence they are communicated to the cavity of the tympanum, and thence to the internal organ. These remarks are added in order to reconcile the present case with the doctrine laid down by Mr. Home in his late paper on the organ of hearing.

Experiments and Observations on the Light which is spontaneously emitted, with some degree of Permanency, from various Bodies. By Nathaniel Hulme, *M.D. F.R.S. and A.S.* Read Feb. 13, 1800. [*Phil. Trans.* 1800, *p.* 161.]

The light which is the object of the present inquiry must be distinguished not only from that which we derive from the sun, but also from the brightness exhibited by artificial phosphori, electricity, meteors, and other lucid emanations. The principal bodies which afford the light here treated of, are, 1) Some vegetable and earthy substances, such as *rotten wood*, and *peat earth*. 2) Marine animals, some in a living state, viz. the *Medusa phosphorea*, the *Pholas*, the *Pennatula phosphorea*, and the *Cancer fulgens*; and most of the marine fishes soon after they are deprived of life. 3) *Animal flesh* in general, some time after the extinction of life. And, 4) Among

insects, the *creeping glow-worm*, the *flying glow-worm*, the *fire-fly*, and the *great lantern-fly*.

The numerous experiments described in this paper were chiefly made on marine animals, particularly herrings and mackerel, which were exposed either in the air, or in water impregnated with different salts, and of different temperatures, in a dark vault to which the Doctor assigns the name of his laboratory.

The results obtained in this investigation are described in nine sections, the summary contents of which are as follows:

1. The quantity of light emitted by putrescent animal substances does not arise from the greater degree of putrefaction in such substances, as is commonly supposed; but, on the contrary, they begin to shine some time before any apparent signs of putrefaction take place, and the greater the putrescence, the less is the quantity of light emitted.

2. Light is a chemical element, or a constituent principle of some bodies, and particularly of marine fishes; and it may be separated from them by a peculiar process, or be retained, and rendered permanent for some time. The experiments from whence this inference is derived were made with pieces of herrings and mackerel, and with living tadpoles immersed in solutions of Epsom salt, sea salt, Glauber's salt, and in all which a quantity of light was manifestly imparted to the saline menstruum, which the latter under various circumstances retained for a considerable time. These experiments seem also to evince, that light is not partially but wholly incorporated with every particle of the animal substance; that it is probably the first elementary principle that escapes after the death of fishes; and that as the putrescence was by no means promoted, but rather retarded by this emission of light, it is highly probable that no offensive putrefaction ever takes place at sea after the death of such myriads of animals as must needs daily perish in the vast ocean, which hence continue long a wholesome food to the many kinds of fish that feed upon their congeneries.

3. Some bodies or substances have a power of extinguishing spontaneous light when it is applied to them. These are water, both pure and impregnated with quick lime, carbonic acid gas, and hepatic gas, fermented liquors, ardent spirits, fixed and volatile alkalies, certain neutral salts, vegetable infusions, pure honey, and the rust of iron, calamine, minium, and manganese dissolved in water.

4. Other bodies or substances have a power of retaining spontaneous light for some time when it is applied to them. These substances are most of the solutions mentioned in the second article, which, when impregnated with some of the lucific matter scraped from herrings and mackerel, retain the light for some days, especially if assisted with some agitation of the phial containing them. The appearances here exhibited are described as being both beautiful and surprising, as they enable us to take light from one substance and transfer it to another, so as to render the latter most brilliantly luminous; or in other words to impregnate a liquid with light.

5. When spontaneous light is extinguished by some bodies or substances, it is not lost, but may be again revived in its former splendour, and that by the most simple means.

These means are in general the greater or less degrees of impregnation of the saline menstrua, which alternately extinguish and revive the light at the option of the operator.

6. Spontaneous light is not accompanied with any degree of sensible heat, to be discovered by a thermometer.

7. Cold extinguishes spontaneous light, but not permanently, as the substances mentioned in the third article; since here the light could be revived in its full splendour, by exposing the substance to a moderate degree of temperature.

8. The eighth section treats of the effects of heat on light when in a state of spontaneous union. We gather from the experiments here described, that in every substance there is a certain point of temperature at which it acquires its maximum of lustre; that this varies considerably in various substances,—the fishes, rotten wood, and aqueous solutions, becoming dark at a temperature of between 96° and 110°; while glow-worms retain their lustre until exposed to the degree of boiling water. Common water impregnated with light, when by mere time and rest, without any considerable change of temperature, it had become obscure, was soon rendered luminous when gradually warmed by small and successive additions of warm water; but no sooner was boiling water added in any considerable quantity, than the luminous appearance vanished, and was altogether extinguished.

It was here observed, that if heat be applied to the bottom of a tube filled with illuminated water which has been some time at rest, the light will descend in luminous streams from the top of the tube to the bottom, and be gradually extinguished.

9. In the ninth and last section we find a set of experiments on the effects of spontaneous light when applied to the human body. As to the *living* body, it was found that on touching the luminous matter of fishes, the light adhered copiously to the fingers and different parts of the hands, remained very lucid for some time, but then gradually disappeared; whereas the same kind of matter being applied to pieces of wood, stone, and the like, of the same temperature as the laboratory, continued luminous on these substances for many hours. It might hence be inferred that the living body ultimately absorbs the light; but other experiments seem to contradict this inference, it having been observed that a piece of shining wood being placed on the palm of the hand, and a glow-worm being breathed upon repeatedly, were both rendered more luminous, and this for a considerable time.

As to the animal fluids; the crassamentum of the blood of healthy persons, and also of some labouring under inflammatory diseases, received indeed the light of a herring to a certain degree, but did not retain it long: and when this coagulum had been kept for some time,

and showed marks of putrescence, the light seemed to be more quickly extinguished. In some instances the light was ejected in globules, like quicksilver when rubbed with any unctuous substance, and afterwards adhered to the sides of the vessel in the form of a lucid ring. The serum both of healthy and diseased persons retained the luminous appearance somewhat longer than the crassamentum, and frequently recovered it when agitated. Urine, both fresh and stale, and bile, showed little disposition to retain this light. Lastly, milk and cream, illuminated by mackerel light, acquired great brightness, and retained it for upwards of twenty-four hours; but when either of these turned sour, they contracted a very extinguishing property, the light in some case vanishing almost instantaneously.

Account of a Series of Experiments, undertaken with the view of decomposing the Muriatic Acid. By Mr. William Henry. *Communicated by the Right Hon. Sir* Joseph Banks, *Bart. P.R.S.* Read Feb. 27, 1800. [*Phil. Trans.* 1800, *p.* 188.]

In the introduction to this paper the author points out the great utility that would accrue to chemical science, were it possible to arrive at a complete analysis of certain acids, since the new, and indeed every system of chemistry, will ever be incomplete and liable to subversion till the particular agents here alluded to have been resolved into their constituent principles. The obstacles, however, which impede the progress of this investigation, are much greater than may appear at first sight; and among these are particularly mentioned the difficulty of obtaining the acids completely separated from all other substances, which, by their presence, will ever tend to introduce uncertainty into the results of the process; it being observed that the attraction between compound particles at all times increases in proportion as we recede from the point of saturation, and that the smallest remnant is often sufficient to perplex all further analysis. The liquid state is thought to be totally unfit for the purpose of this inquiry: and after some other strictures, it is shown that the state of the gas is the only one in which acids can become proper objects of analytical investigation.

In the series of his experiments on the muriatic acid in the gaseous state, Mr. Henry employed the electric fluid as an agent much preferable to artificial heat. "This mode of operating," he says, "enables us to confine accurately the gases submitted to experiment; the phænomena that occur during the process may be distinctly observed, and the comparison of the products with the original gases may be instituted with great exactness." The action of the electric fluid itself, as a decomponent, is no doubt extremely powerful; since it is capable of separating from each other the constituent parts of water, of the nitric and sulphuric acids, of the vegetable alkali, of nitrous gas, and of several other bodies whose components are known to be strongly united. The experiments were

made in straight glass tubes of various diameters, armed at the sealed end with a metallic conductor, through which the shocks were introduced, the gas being at the other end confined by quicksilver.

The experiments, eighteen in number, are arranged under two heads, the first of which relates to the effects of electricity on muriatic gas, either pure or with the admixture of common air and oxygenous gas; and in the second are recorded the effects of electrifying the muriatic acid gas with inflammable substances.

The results of these experiments, which in the first set were generally a diminution, and in the second an expansion of the aerial fluid within certain limits, plainly evinced that no decomposition whatever of the muriatic acid had ever been effected, the residue always exhibiting the characters of hydrogenous or carbonic gases, whence it was reasonable to infer that with all the precautions that were used, the muriatic gas had never been perfectly freed from some admixture of water or other ingredient.

The following general conclusions are deduced from these results.

1) The muriatic acid gas, in the driest state in which it can be produced, still contains a portion of water. The most decisive of the experiments indicating a proportion of 1·4 grain of water to 100 cubical inches of muriatic gas, long exposed to a sufficient quantity of muriatic lime.

2) When electrical shocks are passed through this gas, the watery portion is decomposed; the hydrogen of the water which unites with the electric matter, constituting hydrogenous gas; and the oxygen which combines with the muriatic acid which at the same time acts on the quicksilver, composing muriate of mercury.

3) The electric fluid serves as an intermediate agent in combining oxygen with muriatic acid; while the really acid portion of the muriatic gas does not sustain any decomposition by the action of electricity.

4) When electric shocks are passed through a mixture of carbonated hydrogen and muriatic acid gases, the water held in solution by these gases is decomposed by the carbon of the compound inflammable gas, and carbonic acid and hydrogenous gases are the result.

5) When all the water of the two gases has been decomposed, no effect ensues from continuing the electrization.

6) And lastly, since carbon, though placed under the most favourable circumstances for being separated from the muriatic acid, and combining with its oxygen, evinces no such tendency, it may be inferred, that if the muriatic acid be an oxygenated substance, its radical has a stronger affinity to oxygen than is possessed by charcoal.

Although this investigation have proved unsuccessful as to the particular object for which it was instituted, the author however thinks the communication of it cannot but be productive of some utility; since, besides some material facts it has brought to light, it may prevent others from engaging in most laborious processes of a similar nature, being thus cautioned against the fallacy of their results. All

hope, he apprehends, must be relinquished of ever effecting the decomposition of the muriatic acid in the way of simple elective attraction; its basis being probably some unknown body, which nothing but the application of complicated affinities will perhaps ever enable us to discriminate.

On double Images caused by atmospherical Refraction. By William Hyde Wollaston, *M.D. F.R.S.* Read March 6, 1800. [*Phil. Trans.* 1800, *p.* 239.]

The remarkable instances of double and triple images of the same object produced by aerial refraction near the horizon, lately communicated to the Society by Mr. Huddart, Prof. Vince, and Mr. Dalby, have given rise to the present paper, in which the author attempts to explain these phænomena on theoretical principles, and to illustrate his conclusions by artificial experiments.

Admitting the inference given by Professor Vince, that these appearances arise from certain *unusual variations* of increasing density in the lower strata of the atmosphere, our author undertakes, 1st, to investigate the successive variations of increasing or decreasing density to which fluids in general are liable, and the laws of the refractions occasioned by them; 2dly, to illustrate and confirm the truth of this theory by experiments with fluids of known densities; and lastly, to ascertain, by trial upon the air itself, the causes and extent of those variations of its refractive density on which the inversions of objects and other circumstances observed in the above phænomena seem to depend.

Under the first head we find the demonstrations of three propositions, deduced from the general laws of refraction. The first imports, that if the density of any medium varies by parallel, indefinitely thin strata, a ray of light moving through it in the direction of the strata, will be made to deviate during its passage; and the deviation will ever be proportionate to the increment of density where it passes. From the second it appears, that when two fluids of unequal densities are brought into contact, and unite by mutual penetration, if the densities at different heights be expressed by ordinates to a perpendicular line drawn across the fluids, the curve drawn through the terminations of these ordinates will have a point of contrary flexure. And in the third proposition it is shown, that if parallel rays pass through a medium, varying according to the preceding proposition, those rays above the point of contrary flexure, where the line will be concave, will be made to diverge, while those below the same point, where the curve will be convex, will converge after their passage through it. The converging rays, it follows hence, will at a certain distance, proportionate to the quantity of convergency, meet in a focus, beyond which they will diverge again, and thus produce effects perfectly similar to those caused by a medium of uniform density, having a surface similar to the above-mentioned curve of densities, whether convex or concave, according to the nature of that curvature. Hence may be inferred

the manner in which, according to this theory, an object viewed through a medium of various densities, producing or rather represented by a curve of contrary flexure, and at the same time a contiguous stratum of uniform density, will exhibit three different images; the one through the uniform medium, in its proper place; the other through the convex part, somewhat higher, but inverted; and the third refracted by the concave part, still higher, erect, but somewhat smaller.

Grounded upon these principles, Dr. Wollaston proceeded next to the set of experiments which are the subject of his second section. The first and most explanatory of these experiments was made with a square phial, about one-third filled with clear syrup, and the other third with pure water, the two liquids forming by degrees, at the plane of contact, a thin stratum of decreasing density from the syrup upwards. Here the effect was obviously conformable to the theory,—an object viewed through these media being represented to the eye, erect and in its proper place when seen through the syrup of uniform density; higher and inverted behind the adjacent variable medium; and still higher and erect behind the upper part of the variable stratum. This effect of varied density was repeated by filling the remainder of the phial with spirits of wine; when at the plane of contact between the spirit and the water, another variable stratum was gradually produced, which exhibited the same phænomenon as in the former instance.—The next experiment proves that a difference of temperature between adjacent strata of the same fluid will produce the same effect.—And a third experiment, which may be considered as a corollary, showed that the air round a heated body (a red-hot poker for instance,) will assume the same varied densities, and exhibit precisely the same appearances.

Under the third head the author observes, that though three images have as yet been rarely seen in the atmosphere, yet this circumstance does by no means invalidate the above theory; since its appearing so seldom may be well accounted for by the less rarefaction produced by the heat of the sun, than by a red-hot iron, or the artificial means above used. Over water, the evenness of the surface, he says, is favourable to the production of such appearances.

Some observations are, lastly, added concerning Mr. Huddart's opinion, that the peculiar state of the atmosphere which produced the appearances he witnessed may have been occasioned merely by the evaporation at the surface of the sea condensing the lower strata of the atmosphere. Dr. Wollaston does not altogether accede to this opinion; but he does not absolutely deny that the cold produced by this evaporation may in some instances occasion a density that may enter as one of the data in the theory above laid down, though other causes, such as the effects of the heat of the sun, currents of air, &c. he thinks must co-operate. To the density, however, produced by mere evaporation, he acknowledges may be ascribed the uncommon elevation of the coast of France, lately observed at Hastings by Mr. Latham; and some of the appearances described by Professor

Vince. The depression of the horizon, frequently noticed by persons residing near the sea, and some other phænomena of a similar nature, he thinks may likewise be ascribed to that single cause.

On a new fulminating Mercury. By Edward Howard, *Esq. F.R.S.* Read March 13, 1800. [*Phil. Trans.* 1800, p. 204.]

We learn from the introduction to this paper, that mercury, and most, if not all, its oxides, may, by treatment with nitric acid and alcohol, be converted into a crystallized compound, possessing all the inflammable properties of gunpowder, as well as many others peculiar to itself. After stating the gradual steps by which he arrived at this discovery, Mr. Howard describes the following process and manipulations, which he found best calculated for producing this powder.

One hundred grains, or a greater proportional quantity, of quicksilver, are to be dissolved with heat in a measured ounce and a half of nitric acid. This solution being poured cold upon two measured ounces of alcohol, a moderate heat is to be applied, until an effervescence is excited. A white fume then begins to undulate on the surface of the liquor, and the powder will be gradually precipitated upon the cessation of action and re-action. This precipitate is to be immediately collected on a filter, well washed with distilled water, and carefully dried in a heat not much exceeding that of a water-bath. This immediate edulcoration of the powder is material, it being liable to the re-action of the nitric acid, which, while any of that acid adheres to it, renders it subject to the influence of light. The quantity of the powder produced varies according to the nature of the ingredients; 100 grains of quicksilver yielding from 120 to 132 grains of the compound.

The principal agents which decompose this mercurial powder are the nitric, the sulphuric, and the muriatic acids. The most remarkable effect is that of the sulphuric acid, which, when much concentrated, produces an explosion nearly at the instant of contact, on account, it is thought, of the sudden and copious disengagement of the caloric. When the acid is less concentrated, no explosion takes place; but a considerable discharge of gas, as well as caloric, is nevertheless effected; the former appearing to be a compound of carbonic acid and a peculiar inflammable gas, amounting in the whole to between 28 and 31 cubical inches. The inflammable gas was upon close examination found to be a nitrous ætherized gas, which appears to have been not the result of the decomposition, but, in fact, a constituent part of the powder.

Upon the whole of the investigation, Mr. Howard concludes that this mercurial powder is composed of nitrous ætherized gas, and of oxalate of mercury with excess of oxygen. Having stated his reasons for maintaining this opinion, he goes on to explain the theory of the combustion of the mercurial powder, on certain principles previously laid down in the investigation. The hydrogen, he says, of the oxalic acid and of the ætherized gas is first united to the oxygen of the

oxalate, forming water; the carbon is saturated with oxygen, forming carbonic acid gas; and a part, if not the whole of the nitrogen of the ætherized gas is separated in the state of nitrogen gas; both which gases are evidently produced after the decomposition of the powder. The mercury is now revived, and converted into vapour, as may be gathered from the immense quantity of caloric extricated, by adding concentrate sulphuric acid to the mercurial powder. On a more minute analysis he finds the proportion of these ingredients in 100 grains of the mercurial powder to be as follows:—

		Grains.
Of pure oxalic acid		21·28
Of mercury formerly united to the oxalic acid	60·72	
Of mercury dissolved in the sulphuric liquor	2·00	
Of mercury left in the sulphuric liquor after the separation of the gases	2·00	
Total of mercury..........	——	64·72
Of nitrous ætherized gas and excess of oxygen		14·00
		100·00

The following are the principal properties of this singular powder: —it takes fire at the temperature of 368° of Fahrenheit; it explodes by friction, by flint and steel, and by being thrown into concentrate sulphuric acid. It is equally inflammable under the exhausted receiver of an air-pump, as when surrounded by atmospheric air; and it detonates loudly, both by the blow of a hammer and a strong electrical shock. Its action, though extremely powerful, is however confined within a very limited sphere. It will burst a gun-barrel, though it will not carry a ball to any considerable distance.

Mr. Howard does not fail to caution future operators concerning the experiments they may be tempted to make on this powerful agent, having himself suffered considerably from an instantaneous explosion, produced by pouring six drams of concentrated sulphuric acid upon fifty grains of the powder, which wounded him severely, and destroyed the best part of his apparatus. This uncommon elastic power is ultimately ascribed to the gas and caloric suddenly set at liberty, and to the mercury and some portion of water being converted into vapour.

The paper concludes with some observations on other fulminating powders, where the author acknowledges that he has in vain attempted to communicate fulminating properties, by the *mercurial process*, to gold, platina, silver, antimony, tin, copper, iron, lead, zinc, nickel, bismuth, cobalt, arsenic, and manganese; mercury being as yet the only metal which he has found to have a joint affinity with nitrous ætherized gas and oxalic acid, or to be capable of combining with nitrous ætherized gas.

Investigation of the Powers of the prismatic Colours to heat and illuminate Objects; with Remarks, that prove the different Refrangibility of radiant Heat. To which is added, an Inquiry into the Method of viewing the Sun advantageously, with Telescopes of large Apertures and high magnifying Powers. By William Herschel, *LL.D. F.R.S.* Part I. Read March 27, 1800. [*Phil. Trans.* 1800, p. 255.]

After recommending a cautious circumspection in admitting specious appearances and plausible inferences in our researches both after physical and moral truth, the Doctor acknowledges that a general diffidence of this nature had raised a doubt in his mind, that the power of heating and illuminating objects is not equally distributed among the various coloured rays. This surmise received some confirmation from the different sensations he experienced on viewing the sun with his large telescopes, and through various combinations of differently coloured glasses. With some of these combinations he felt a sensation of heat, though he had but little light; while others gave much light, with scarce any degree of heat. Suspecting hence that perhaps certain coloured rays may be more apt to occasion heat, while others, on the contrary, may be more fit for vision, he resolved to put this conjecture to the test of experiments.

The first set of these experiments relate to the *heating* power of coloured rays. They were made by admitting successively each differently coloured ray of a prismatic spectrum, through a proper aperture in a pasteboard, on a thermometer whose bulb was blackened, while another similar thermometer, at a certain distance, showed the temperature of the ambient air. The general results here were, that the temperature, or rather the power of heating of the red ray, is greater than any other, bearing a proportion to that of the green ray as 9 to 4, and to the violet, the least calorific, as 13 to 4.

The next series of experiments was on the *illuminating* power of coloured rays. These were simply made by viewing through a microscope certain opaque bodies, consisting of minute particles, and illuminated successively by different coloured rays. These substances were red, green, and black paper, a piece of brass, a nail, and a guinea.

The uncommon variegated appearances of the metals, and especially of the iron nails, occasioned by their very minute and differently arranged particles, is here mentioned both as an object of admiration, and as singularly conducive to the purposes of the present inquiry; the greater or smaller number of these particles that became discernible by the different coloured rays affording a kind of scale of comparison which led to the inferences here laid down.

These general inferences are, that the red-making rays are very far from having the illuminating power in any eminent degree; that the orange possesses more of it than the red, and the yellow still more; that the maximum of illumination lies in the brightest yellow or palest green; that the green itself is nearly equally bright with

the yellow; but that, from the full deep green, the illuminating power decreases very sensibly, the blue being nearly upon a par with the red, while the violet is still more, and the purple the most deficient of any.

From a collective view and a due contemplation of these well established facts, it is inferred, that perhaps they may lead to the discovery of different chemical properties in the different coloured rays; and that the various degrees of heat in different coloured flames will probably be fully explained on the principles that may be deduced from them.

Here follows a section in which the author advances a position, that radiant heat is of different refrangibility, and that it is subject to the laws of the dispersion arising from this different refrangibility. We perceive from the drift of his arguments here advanced, that he inclines in favour of a distinction between *calorific* rays and *colorific* light, both being liable to refraction, but under different angles, and hence probably possessed of different momenta. This, if admitted, will apply to the invisible heat of red-hot iron gradually cooled till it ceases to shine, and likewise affords a solution of the reflection of invisible heat by concave mirrors.

We come next to an application of the result of the foregoing observations to the method of viewing the sun advantageously, with telescopes of large apertures and high magnifying powers. It is well known, that even with the naked eye, and much less with telescopes of large apertures, the sun cannot be viewed without a darkening apparatus. Dr. Herschel had repeatedly used red glasses for this purpose; but always found, that though the lustre was thereby sufficiently abated, yet they did not prevent an irritation on the eye, which was manifestly the effect of heat. This induced him to make experiments with glasses of various other colours, the result of which was, that, as might have been inferred from the above investigation, dark-green glasses are the most efficacious for intercepting the red or more calorific rays, and will therefore answer one of the purposes of the darkening apparatus. But as in viewing the sun we have, besides the heat, also its splendour to contend with, further trials were made to obviate this inconvenience: and for this purpose, common smoked glasses were found the most efficacious. Some directions are here given for smoking glasses uniformly, and in the most convenient manner.

From a series of telescopic experiments respecting this darkening apparatus, which concludes the paper, we learn, that as the heat will often crack the glasses when placed at or near the focus of the pencils of rays, a safer and more advantageous way of applying them is to place them before and near the small speculum, or immediately behind the second eye-glass. In this last position a single dark-green glass was found of great utility; but a deep blue glass, with a blueish-green smoked one upon it, was still preferable: the sun appeared of a whiter colour than with any other composition, and the sensation of heat was by no means inconvenient.

Experiments on the Refrangibility of the invisible Rays of the Sun. By William Herschel, *LL.D. F.R.S.* Read April 24, 1800. [*Phil. Trans.* 1800, p. 284.]

In a paper read to the Society at a former meeting, the Doctor announced some observations which seemed to indicate that there are two sorts of rays proceeding from the sun; the one the calorific rays, which are luminous and refrangible into a variegated spectrum; and the other the invisible rays, which produce no illumination, but create a sensible degree of heat, and appear to have a greater range of refrangibility than the colorific rays. To the latter he assigns the name of *radiant heat*. Having lately had some favourable opportunities to prosecute this investigation, he here delivers an account of the series of experiments he made on the subject, which seem to him to confirm the above conjecture. The mode of conducting these experiments was simply this:—

On a horizontal tablet covered with white paper, and divided into squares, for the conveniency of measurement, a part of the extreme colour of a prismatic spectrum was suffered to fall, the remainder of the coloured rays passing by the edge of the tablet, so as not to interfere with the experiment.

Three thermometers were placed on the tablet, at different distances from the termination of coloured rays. The general results of the ten experiments here described were as follows:—

From the four first it appears, that there actually are rays coming from the sun which are less refrangible than any of those which affect the sight; that they are invested with a high power of heating bodies, but with none of illuminating objects, which probably is the reason why they have hitherto escaped unnoticed.

The fifth and sixth experiments showed that the power of heating is extended, though in a feeble degree, to the utmost limits of the most refrangible or visible purple rays, but not beyond them; and that it is gradually increased as the coloured rays grow less refrangible. And from the four last experiments we gather that the maximum of the heating power resides among the invisible rays without the prismatic spectrum, and is probably about half an inch beyond the last visible one, or from the confines of the red ray. These likewise show that the sun's invisible rays, in their less refrangible state, still exert a heating power, considerably beyond this maximum, fully equal to that of the red-coloured light; and that consequently, if we may infer the quantity of the *efficient* from the *effect* produced, these invisible rays of the sun probably far exceed the visible ones in number.

The inferences deduced from these results are, that the range of refrangibility of radiant heat, or colorific rays, when dispersed by a prism, begins at the purple-coloured light, where they are most refracted, and have the least efficacy; and that their refrangibility lessens and their power increases as they approach the confines of the red-coloured light, but that these confines are not the limits of

their decreasing refrangibility and increasing power, these having been traced far beyond the prismatic spectrum in an invisible state; *that as their density gradually decreases, their energy at last vanishes,* till at length the thermometrical spectrum, as the Doctor is willing to call it, becomes wholly imperceptible. Hitherto the effects of these heating rays have been observed as far as one inch and a half from the confines of the red ray.

If this be a true account of solar heat, (says our author at the close of his paper,) it remains only for us to admit, that such of the rays of the sun as have the refrangibility of those which are contained in the prismatic spectrum, by the construction of the organs of sight, are admitted under the appearance of light and colours; and that the rest, being stopped in the coats and humours of the eye, act upon them, as they are known to do upon all the parts of our body, by occasioning a sensation of heat.

Experiments on the solar, and on the terrestrial Rays that occasion Heat; with a comparative View of the Laws to which Light and Heat, or rather the Rays which occasion them, are subject, in order to determine whether they are the same, or different. By William Herschel, *LL.D. F.R.S.* Read May 15, 1800. [*Phil. Trans.* 1800, p. 293.]

In the prefatory part of this paper, the author found it necessary to limit the sense he affixes to the word *heat*; and after excluding the late terminology of *latent, absolute, specific, sensible* heat, the *matter of heat, caloric,* and even *radiant heat,* which last, however, comes nearest to the expression he has adopted, he desires to be understood, that, in speaking of *rays which occasion heat,* he does not mean that those rays themselves are heat, but that he here considers heat merely as the effect of a cause, the nature of which is no part of his present inquiry.

Having thus determined the subject of his investigation, the Doctor distinguishes heat into six different kinds; whereof three are solar, and three terrestrial. These, however, are reducible into three general divisions, each of the solar and terrestrial kinds resembling each other respectively. The first is the heat produced by luminous bodies, whether by the sun or by terrestrial flames. The second comprehends the heat of coloured radiants, such as that of the sun separated by a prism, and that of culinary fires openly exposed. And the third relates to heat from radiants, where neither light nor colour can be perceived; such as the heat of invisible solar rays, refracted by a prism, which have been the subject of a former paper; and the terrestrial heat from fires inclosed in stoves, and from metals heated short of the lowest degree of incandescence.

The chief object of the present inquiry being to give a comparative view of the operations that may be performed on the rays that occasion heat, and of those which we know to have been effected on the rays that occasion light, a short detail is given of the principal facts

respecting the latter, which not only are well known to be founded, but are also best calculated to elucidate the comparison. These are the seven following:—1. Light, both solar and terrestrial, is a sensation occasioned by rays emanating from luminous bodies; 2. These rays are subject to the laws of reflection; 3. They are refrangible; 4. They are of different refrangibility; 5. They are liable to be detained by different diaphanous bodies; 6. They are liable to be scattered on rough surfaces; and 7. They have hitherto been supposed to have a power of heating bodies, which however remains as yet to be examined.

The similar propositions respecting heat which the Doctor intends to prove, are as follows:—1. Heat, both solar and terrestrial, is a sensation occasioned by rays emanating from *candent* substances; 2. These rays are subject to the laws of reflection; 3. They are refrangible; 4. of different refrangibility; 5. liable to be detained in their passage through other bodies; 6. liable also to be scattered on rough surfaces; and lastly, They may be supposed, when in a certain state of energy, to have a power of illuminating objects; which last, however, remains as yet to be examined.

The paper before us is limited to the experiments on the three first of the above-mentioned comparative propositions. They are twenty in number, of which the ten first relate to the *reflection*, and the ten last to the *refraction* of these rays, under all the variety of circumstances deducible from the different kinds of heat above enumerated; to which are added, some attempts to produce a condensation of heat independent of light, by spherical mirrors and lenses. Such mirrors and lenses, together with accurate thermometers, were the instruments used in these experiments, of which those on invisible solar heat, and invisible culinary rays, are perhaps the most striking, as they serve to corroborate the theory laid down by the Doctor in a former paper concerning the existence of such heat and rays independent of light.

It being impracticable to epitomize the ample account of these experiments given in the paper, we must content ourselves with observing in general, that all their results fully evince the truth of the second and third propositions above laid down, viz. that the rays which occasion heat, both solar and terrestrial, in all their different kinds, and under every variety of circumstances that could be devised, are subject to the laws of reflection and refraction.

The same results also convey sufficient evidence of the radiant nature of light; and hence equally prove the first of those propositions. The three following ones, viz. the fourth, fifth, and sixth, are reserved for a future communication; where the author proposes likewise to enter into a discussion concerning the seventh or last of them, relating to the power of heating and illuminating.

Chemical Experiments on Zoophytes; with some Observations on the component Parts of Membrane. By Charles Hatchett, *Esq. F.R.S.* Read June 12, 1800. [*Phil. Trans.* 1800, p. 327.]

Having completed the series of experiments on the component parts of shell and bone which he described in a former paper, it was suggested to Mr. Hatchett that there still remained a large class of substances belonging to the animal kingdom, namely, the various species of Zoophytes, which had never yet been carefully analysed, and the investigation of which would probably lead to some curious, and perhaps useful inferences. Although aware of the extensiveness of the inquiry, he yet readily engaged in it; and we have now before us the fruit of his indefatigable industry and ardent zeal for the advancement of science.

The first part of the paper contains a full account of the series of experiments he made on the abovementioned substances, which, in the present pressure for time, we must pass over unnoticed in order to hasten to the second, which consists of observations on those experiments, in the course of which the subject necessarily led to an inquiry into the nature and properties of several other analogous substances, such as horn, nail, hoof, quill, hair, feathers, tortoiseshell, the scales of fish, amphibious animals and insects, albumen, and even muscular fibre.

We must here recollect that in his former paper on shell and bone Mr. Hatchett had arrived at the conclusion that their essential ingredients were carbonate of lime in the former, and phosphate of lime in the latter, the bases in both consisting of different modifications of a glutinous, gelatinous, or membranaceous substance. This conclusion he had the satisfaction of seeing corroborated, and the chain of connexion widely extended, by the facts deduced from the present inquiry. The general results of which are, that the Madrepores and Millepores (like several of the shells) are formed of a gelatinous or membranaceous substance, hardened by carbonate of lime, the difference consisting only in the mode in which these materials are combined: that in the Tubipora, Flustra and Corallina, some phosphate of lime is mixed with the carbonate of lime: that in the Isis the basis is a regularly organized membranaceous, cartilaginous and horny substance, hardened by carbonate of lime, one species only (the Isis ochracea) yielding also a small proportion of phosphate of lime. That the hardening substance of the Gorgonia nobilis is likewise the carbonate of lime, with a small portion of phosphate; but that the matter forming the membranaceous basis consists of two parts, the interior being gelatinous, and the external a complete membrane, so formed as to cover the stem in the manner of a sheath or tube. That the other Gorgoniæ consist of a horny stem coated by a membrane, which is hardened by carbonate of lime. That the Sponges are of a nature similar to the horny stems of the Gorgoniæ, and only differ from these and from each other by the quality of texture. And lastly, that the Alcyonia are likewise composed of a soft

flexible membranaceous substance, very similar to the cortical part of some of the Gorgoniæ; and in like manner slightly hardened by carbonate, mixed with a small portion of phosphate of lime.

From this mass of evidence we collect, in general, that the varieties of bone, shell, coral, and the numerous tribe of Zoophytes with which the last are connected, only differ in composition by the nature and quantity of the hardening or ossifying principle, and by the state of the substance with which this principle is mixed or connected; the gluten, or jelly, which cements the particles of carbonate or phosphate of lime, and the membrane, cartilage, or horny substance which serves as a basis, appearing to be only modifications of the same substance, which progressively graduates from a viscid liquid, or gluten, into a gelatinous substance, which again, by increased inspissation, and by the more or less perfect degrees of organic arrangement, forms the varieties of membrane, cartilage, and horn, which it seems form the peculiar differences of the several species.

It is obvious that in this inquiry much depends upon an accurate investigation of the gluten, or jelly, so often mentioned as a principal ingredient in the substances under examination. This gave rise to the experiments on the analogous substances above mentioned, which led to a better acquaintance with the substance which now obtains the name of *gelatin*.

Not being allowed to enter into a detail of these experiments, we shall only observe at present, that this gelatin is a component part of most of the animal substances above enumerated; that it varies in quality from a very attenuated jelly or mucilage, to that viscid substance called glue, the varieties of which also differ in solubility and tenacity: that it is present in various proportions, so that certain bodies, such as the cutis and cartilages of the articulations, seem to be entirely formed by it; while others, like nail, quill and tortoise-shell, can hardly be said to contain any; and that by its presence in various states and proportions, it may be regarded as the principal cause of those degrees of flexibility, of elasticity and of putrescibility, so various in different parts of animals.

In all these substances, when all the gelatin they contained had been separated, either by repeated boiling in water, or by being steeped in dilute acids, a more insoluble substance remained of a very different nature from the gelatin, and which became the object of another extensive analysis. The results here led to the curious and important conclusion, that the substance known by the name of albumen is in fact the primary animal matter from which all the others, and even the gelatin and the animal fibre, are ultimately derived, the formation of the two latter beginning with the process of sanguification in the fœtus, and the immense variety in the animal creation being deducible from the infinite diversity and modifications in texture, flexibility, elasticity, and other properties of the same substance composing the several parts which constitute the bodies of animals.

On the Electricity excited by the mere Contact of conducting Substances of different Kinds. In a Letter from Mr. Alexander Volta, *F.R.S. Professor of Natural Philosophy in the University of Pavia, to the Rt. Hon. Sir* Joseph Banks, *Bart. K.B. P.R.S.* Read June 26, 1800. [*Phil. Trans.* 1800, p. 403.]

In prosecuting his experiments on the electricity produced by the mere contact of different metals, or of other conducting bodies, the learned Professor was gradually led to the construction of an apparatus, which in its effects seems to bear a great resemblance to the Leyden phial, or rather to an electric battery weakly charged; but has moreover the singular property of acting without intermission, or rather of re-charging itself continually and spontaneously without any sensible diminution or perceptible intervals in its operations. The object of the present paper is to describe this apparatus, with the variety of constructions it admits of, and to relate the principal effects it is capable of producing on our senses.

It consists of a long series of an alternate succession of three conducting substances, either copper, tin and water; or, what is much preferable, silver, zinc, and a solution of any neutral or alkaline salt. The mode of combining these substances consists in placing horizontally, first, a plate or disk of silver (half-a-crown, for instance,) next a plate of zinc of the same dimensions; and, lastly, a similar piece of a spongy matter, such as pasteboard or leather, fully impregnated with the saline solution. This set of three-fold layers is to be repeated thirty or forty times, forming thus what the author calls his *columnar machine*. It is to be observed, that the metals must always be in the same order, that is, if the silver is the lowermost in the first pair of metallic plates, it is to be so in all the successive ones, but that the effects will be the same if this order be inverted in all the pairs. As the fluid, either water or the saline solution, and not the spongy layer impregnated with it, is the substance that contributes to the effect, it follows that as soon as these layers are dry, no effect will be produced.

This apparatus, when it consists of only twenty pairs of metallic plates, is already capable not only of giving to Cavallo's electrometer, with the aid of a condenser, signs of electricity as high as 10° or 15°, and of charging the condenser by a simple touch to such a degree as to give a spark; but it will also give to two fingers of the same hand, the one touching the foot and the other the top of the column, a succession of small shocks, resembling those occasioned by a Leyden phial, or a battery weakly charged, or by a torpedo in a weak condition. These effects will be increased if the communication be made through water; for which purpose the bottom of the column may be made to communicate, by a thick metallic wire, with water contained in a basin or large cup. A person who now puts one hand into this water, and with a piece of metal held in the other hand touches the summit of the column, will experience shocks and a pricking pain as high as the wrist of the hand plunged in the water, and even some-

times as high as the elbow, while in some cases even the wrist of the other hand will experience a similar sensation.

It has been ascertained by repeated trials, that these effects are stronger in proportion to the greater distance of the metallic pairs, which are made to communicate. Some sensation will be produced when the foot of the column is connected with the third or fourth pair, but it will perceptibly increase as we proceed further towards the summit. This naturally led to an extension of the column much beyond the number of metallic pairs above mentioned; and expedients are here suggested for rendering such extended columns stable and at the same time sufficiently manageable. With a column of about sixty pairs of plates, shocks have been felt as high as the shoulder; such a column may be even divided into two or three distinct cylinders, which being well connected by metallic conductors, will be equally powerful and much more convenient.

Among various other modes of applying the same agents, the author describes an apparatus in which the fluid is interposed between the metals without being absorbed in a spongy substance. This consists of a number of cups or goblets, of any substance except metals, placed in a row either straight or circular, about half filled with a saline solution, and communicating with each other so as to form a kind of chain, by means of a sufficient number of metallic arcs or bows, one arm of which is of silver, or copper plated with silver, and the other of zinc. The ends of these bows are plunged into the liquid in the same successive order, namely, the silver ends being all on one side, and those of zinc on the other,—a condition absolutely necessary to the success of the experiments, it having been observed that if out of sixty bows, for instance, the twenty intermediate ones be turned in the opposite direction from the remainder, the effects produced by the apparatus will be *far less* perceptible.

It was observed, that if a circular communication be completed by means of a bow connecting the first and last of a long series of cups, two hands, or even two fingers plunged into one of these cups will still receive an electric sensation. This is explained by admitting the fact, that warm animal substances, and particularly their fluids, are in general better conductors than water.

The sensible effects of either of these apparatus, composed of forty or fifty links, do not, it seems, consist merely in shocks, contractions, or spasms in the muscles or limbs; but, besides affecting the sense of touch, they are also capable of exciting an imitation in the organs of taste, sight, and even hearing. A particular account is given of these singular effects, from which we learn, that the more sensible the parts are which are exposed to the impressions of this agent, the more quick will be the sensation;—that as to taste, we have only to recollect the experiments formerly described by the author, in which the tongue was sensibly affected by the combination of two metals applied to each side of it;—that with respect to the sense of vision, the sparks yielded by this apparatus are sufficient evidence of the effect, certain expedients only being necessary for facilitating the

perception of these explosions;—and lastly, that the hearing will be strongly affected by introducing into the ears two probes, the opposite extremities of which are connected with the two ends of the apparatus. No effect has as yet been produced upon the sense of smell by this machine, which is ascribed to the circumstance of the electric effluvia not being expanded in and conveyed by the air, which it is thought is the proper vehicle for exciting sensations in the olfactory nerves.

At the close of the paper the author points out the striking analogy there is between this apparatus and the electric organs of the torpedo and electric eel, which are known to consist of membranaceous columns filled from one end to the other with a great number of laminæ or pellicles, floating in some liquid which flows into and fills the cavity. These laminæ cannot be supposed to be excited by friction, nor are they likely to be of an insulating nature; and hence these organs cannot be compared either to the Leyden phial, the electrophore, the condenser, or any other machine capable of being excited by friction. As yet, therefore, they can only be said to bear a resemblance to the apparatus described in this paper. The effects hitherto known of this apparatus, and those which there is every reason to expect will be discovered hereafter, are likely, it is thought, to open a vast field for reflections and inquiries, not only curious but also interesting, particularly to the anatomist, the physiologist, and the physician.

Some Observations on the Head of the Ornithorhynchus paradoxus. *By* Everard Home, *Esq. F.R.S.* Read July 3, 1800. [*Phil. Trans.* 1800, p. 432.]

We learn from this communication that the beak of this singular animal, which on a cursory examination was thought to be exactly similar to that of the Duck, and calculated for the same purposes, is in fact materially different from it; and that, so far from being the mouth of the animal, as had been imagined, it is only a part added to the mouth, and projecting beyond it. This mouth has two grinding teeth on each side, both in the upper and lower jaw; they are without fangs, and may be considered as bony protuberances. Instead of incisor teeth, the nasal and palate bones are continued forwards, so as to support the upper portion of the beak; while the two under jaws are likewise continued forwards in the shape of two thin plates of bone, forming the central part of the under portion of the beak. The tongue is very short, and when extended can be projected into the bill scarcely one quarter of its length.

The organ of smell in this animal differs from that of quadrupeds in general, as well as of birds. The nostrils are nearly at the end of the beak, while the turbinated bones are situated in the skull, as in other quadrupeds; by which means there are two cavities the whole length of the beak superadded to this organ. The nerves which supply this organ are very large in proportion to the size of the animal.

Considering this curious structure of the nose in an animal which lives in water, it is natural to conclude that nature has fitted it for discovering its prey in that element, by means of the sense of smell; and that for this purpose it is enabled to introduce this prominence into the small recesses in which its natural food is probably concealed.

Experiments on the solar, and on the terrestrial Rays that occasion Heat; with a comparative View of the Laws to which Light and Heat, or rather the Rays which occasion them, are subject, in order to determine whether they are the same, or different. By William Herschel, *LL.D. F.R.S.* Part II. Read November 6, 1800. [*Phil. Trans.* 1800, p. 437.]

In the first part of this paper * the Doctor had proposed the seven following points which he meant to elucidate in this inquiry. 1. That heat, both solar and terrestrial, is a sensation occasioned by rays emanating from candent substances. 2. That these rays are subject to the laws of reflection. 3. That they are refrangible. 4. That they are of different refrangibility. 5. That they are liable to be detained in their passages through other bodies. 6. That they are also liable to be scattered on rough surfaces. And lastly, he proposed to ascertain whether in a certain degree of energy these rays may not have or acquire a power of illuminating objects. The three former points have been considered in the first, and the four last are the subjects of the present part of the paper.

Concerning the different refrangibility of the rays of heat, being the subject of the fourth article, we find that in refracting the rays of the sun by a prism, two distinct spectra may be said to be produced, the one of light, and the other of heat, the latter being distinctly observable by means of thermometers. These two spectra the Doctor has found means to represent by a figure, in which the length of the luminous or coloured spectrum, being represented by a line on which are raised ordinates proportionate to the quantity of illumination of each coloured ray, the curve joining these ordinates, together with this base line, inclose an area which may be said to represent the extent and intensity of the coloured rays. Adopting now another base line of the length of the range of the refracted rays of heat, one extremity of which is found to coincide with the termination of the coloured spectrum at the outward edge of the violet ray, and the other to project beyond the opposite termination at the red ray, which makes this line longer than the other, in the proportion of nearly 5½ to 3. Ordinates are here in like manner applied according to the different degrees of intensity of heat indicated by accurate thermometers, and thus another area is produced, which represents the spectrum of heat both as to extent and intensity. On inspecting these figures, parts of which coincide, but other parts considerably deviate from each other, we find that the coloured and the heating rays differ widely, both in their mean refrangibility and

* Page 20.

in the situations of their maxima. And we also collect that the projecting part of the heating rays, being on the side of the red or least refrangible coloured ray, the aggregate of the former may be said to be less refrangible than that of the latter.

The Doctor now goes on to prove by experiments, that the sines of refraction of the heat-making rays are in a constant ratio to their sines of incidence, and points out the results of a correction of the different refrangibility of heat, by contrary refractions in different media.

Experiments are also described which show that the focus produced by a lens is in fact twofold, that which is produced by the rays of heat being in the same axis, but at some distance further from the lens than the luminous focus,—a property that might have been inferred, *à priori*, from the less refrangibility of the heat-making rays.

In the fifth article, which treats of the transmission of heat-making rays through diaphanous bodies, besides the accurate description of the various apparatus which it was necessary to contrive for the purpose, and which can only be clearly understood by inspecting the figures added to the paper, we find the results of 170 experiments distinguished under the six following sections. 1. On the transmission of solar heat through colourless substances; through glasses of the different prismatic colours; through liquids, such as well and sea-water, and different spirits; and through scattering substances, such as ground glasses, paper, linen, silk, &c. 2. On the transmission of the heat of terrestrial flame through various substances. 3. On the transmission of the solar rays, which are of an equal refrangibility with the red prismatic rays. 4. On the transmission of fire-heat through various substances. 5. On the transmission of invisible rays of solar heat. And lastly, (the subject which appears most pregnant with useful inferences for the common purposes of life,) on the transmission of invisible terrestrial heat. Not only the general position, that the rays of heat, both solar and terrestrial, are detained in their passage through various bodies, appears to be here completely evinced, but the great variety in the power of the transmitting bodies seems also to be determined with abundance of accuracy, and affords matter of much consideration and curiosity.

From the sixth article, in which it is intended to prove that the rays of heat, both solar and terrestrial, are liable to be scattered on rough surfaces, it appears that all bodies, even the most polished, are sufficiently rough to scatter heat in all directions. And the chief object of the twenty-four experiments here described, is to compare the effects of rough surfaces on heat with their simultaneous effects on light. The general and rather unexpected result is here brought forward, that colours have no concern whatever in the laws that relate to the scattering of heat.

The chief object of the whole of this inquiry follows next in the seventh article, where the question is discussed, "Whether light and heat be occasioned by the same or by different rays?" One of the leading facts deduced from the experiments in the fourth section, is that there are rays of heat, both solar and terrestrial, not endowed

with a power to render objects visible, and that there are different degrees of heat in the prismatic spectrum of these invisible rays. This being established, the question now, according to the original enunciation, is in fact, "Whether some of these heat-making rays may not have a power of rendering objects visible, superadded to their own already established power of heating bodies?" From a general and comparative view of those among the preceding experiments which apply to this question, we gather that no kind of regularity takes place among the proportions of the luminous and heating rays which are stopped in their passage, and that hence it might be reasonably inferred that heat and light are entirely unconnected. Yet, not to evade the above hypothesis, the Doctor enters into a more minute investigation of the subject, and shows that, admitting, according to the supposition, that the same rays being both luminous and calorific, may in their passage through certain media be so affected as to produce the very discordant results observed in the experiments, it is yet evident, on a due comparison of those results, that no given proportion that may be ascribed to this operation of the transmitting media, will anyways account for the general phænomena; the degrees of heat being in some instances greatly redundant, and in others as much deficient, both ways deviating from any given proportion. Thus it is that he reduces his opponent to the dilemma of either maintaining that the same agent may under different circumstances produce effects perfectly dissimilar, such as heat without light, decreasing heat and increasing light, or the reverse; or else to admit that there actually is a difference between the rays that give light, and those which produce heat.

A more direct proof of the difference of the two sorts of rays is deduced from the manifest results of the experiments, in which the stoppage of one sort of rays does by no means occasion the stoppage of the other sort. In investigating this subject the Doctor controverts a conjecture that the phænomena observed may be ascribed to a peculiar texture or configuration in the diaphanous substances, which produce differences in the transmission of the rays, though there be no difference in the rays themselves. This hypothesis also is minutely investigated, and its contradiction with the experiments being pointed out, its very foundation seems in fact to be wholly subverted.

Lastly, another direct proof of the difference of the two sorts of rays, is deduced from a comparative view of the results of some of the experiments, from which it appears that the stoppage of heat is in general gradually extending as far as five minutes in time, whereas the suppression of light hitherto appears to be instantaneous. This, together with various other arguments derived from the transmission of terrestrial heat, which cannot be properly explained in a manner sufficiently concise for this place, seem to evince that in fact the law by which heat is transmitted is essentially different from that which directs the passage of light, and that hence there is every reason to believe that the rays of heat are different from those of light.

An Account of the Trigonometrical Survey, carried on in the Years 1797, 1798, *and* 1799, *by Order of* Marquis Cornwallis, *Master-General of the Ordnance. By Captain* William Mudge, *of the Royal Artillery, F.R.S. Communicated by His Grace the* Duke of Richmond, *F.R.S.* Read July 3, 1800. [*Phil. Trans.* 1800, *p.* 539.]

The mode of conducting this important survey having been already noticed in the Journals of the Society on various former occasions, it will only be necessary here to state the progress of the operation, which we find has now been carried on over Essex, the western part of Kent, Suffolk, and Hertfordshire, and portions of the counties contiguous to them. A distinct section contains the calculations of the sides of the principal and secondary triangles extended over the country in the three abovementioned years, together with an account of the measurement of a new base-line on Sedgemoor, and a short historical narrative of each year's operations. Another section contains the computed latitudes and longitudes of the places on the western coast intersected in 1795 and 1796, and also of such others since determined as lie conveniently situated to the newly observed meridians. Here we find likewise the directions of those meridians; one on Blackdown in Dorsetshire, another on Butterton Hill in Devonshire, and another on St. Agnes Beacon in Cornwall; as also the bearings, distances, &c. of the stations and intersected objects from the several ascertained parallels and meridians.

The Croonian Lecture. On the Irritability of Nerves. By Everard Home, *Esq. F.R.S.* Read Nov. 20, 1800. [*Phil. Trans.* 1801, *p.* 1.]

Its object is principally to investigate the opinion hitherto entertained, that the nerves may be considered as chords that have no power of contraction within themselves, but only serve as a medium by means of which the influence of the brain may be communicated to the muscles, and the impressions made upon the different parts of the body may be conveyed to the brain. After pointing out the extreme difficulty of such an inquiry, owing to the few opportunities that offer for investigating the real state of the nerves in the living body, Mr. Home intimates that he resolved to avail himself of every opportunity that might offer of any operation in surgery performed upon nerves, either in a healthy state, or under the influence of disease, in order to elucidate this intricate point, without neglecting certain experiments he thought he could devise upon animal bodies, before they are wholly deprived of life.

The first case, which explains some circumstances respecting the actions of the nerves when under the influence of disease, was that of a middle-aged person, who, having hurt his thumb by a fall, experienced long after an occasional swelling and convulsions in that part, attended with spasms, which at times extended in the direct course of the trunks of the radial nerve up to the head, the patient being at times afflicted with absolute insensibility. In order to put a stop to

the progress of this irritation, which seemed to constitute the disease, it was proposed to divide the nerve as it passes from under the annular ligament of the wrist. This operation was accordingly performed, but not altogether with the desired success, owing probably to the wound not healing by the first intention.

Among several singular circumstances in this case, we find also a retraction which took place in the cut ends of the nerve at the time of the operation; and the first idea that suggested itself, was to endeavour to ascertain whether this effect arose from an increase of a natural action in the nerves, or of one newly acquired and entirely produced by the disease. Various experiments were accordingly made on the cutaneous nerves of rabbits, and the phrenic nerve of horses immediately after they were killed. The results were, that a considerable retraction *always* took place, and that any action the nerves are capable of exciting is nearly as strong after apparent death has taken place from a violence committed upon the brain (the horses having been killed by such means), as while the animal is in perfect health.

As it might be suspected that the cellular membrane is the agent by which this retraction of the divided nerves is produced, further experiments were made in such a way as to prevent any other surrounding part from acting upon the nerve. Other experiments were also attempted, with a view to determine whether the power of contraction in a nerve continues for any length of time after apparent death has taken place, and also to ascertain what proportion of elasticity may be possessed by the nerves. The results were,—1. That in all cases the nerves of an animal in health are capable of retracting themselves when divided, and that this effect is entirely independent of the parts by which they are surrounded. 2. That this contraction takes place in the nervous fibres themselves, and is independent of the brain from which they originate, and the muscles and other parts in which they terminate. 3. That the contracted nerve exhibits to the eye an appearance of contraction (a serpentine or undulated direction), which does not appear when the nerve is in a relaxed state.

As the nerves are so easily influenced by electricity in exciting the muscles to action, it naturally suggested itself that some further information might be obtained in the present investigation, by means of experiments made upon the nerves by the electric fluid. From a number that were made for the purpose, it appeared that when the nerve had been previously contracted in consequence of being divided, no increase of that contraction was produced by the electric fluid, nor was any sensible effect perceived when the nerve had not been previously irritated.

During this investigation a singular case occurred, from which it appears that electricity is capable of depriving a nerve of all its functions for a certain time, without destroying them entirely. A young woman, while in the act of bolting the window-shutter, was struck across the eyes by lightning, and a second flash struck her a few minutes after, while she was rubbing her eyes with her hand having a

thimble on one of the fingers. This deprived her of sight for ten days, after which, having had a severe hysterical paroxysm, her vision returned, but convulsive and hysterical sensations still remained, from which she was not relieved till six weeks after. This case, which the author conceives to be a proof that the electric fluid is capable of suspending the functions of the optic nerve without altogether destroying them, suggested the idea of some further experiments, with a view to ascertain whether electricity could be so applied, by artificial means, as to destroy the power of contraction possessed by nerves. The effects seemed to prove the negative; but it is owned these experiments were made under circumstances which did not inspire much confidence.

It appears, in general, that these experiments, and the observations deduced from them, materially illustrate an action or power inherent in the nervous chords, capable of producing the symptoms which occurred in the cases here related; and that the once favourite hypothesis of a nervous fluid, does not give a satisfactory solution of those nervous agitations, which only proceed for some way in the course of a nerve, and are there arrested without being allowed to proceed to the brain. The disorder known by the name Tic Douloureux, is given as an example of the manner in which spasmodic tremors are propagated along the nerves. And a case of a locked jaw occasioned by an injury to the thumb, is lastly mentioned, which corroborates all that has been said concerning the first case mentioned in this paper.

The Bakerian Lecture. On the Mechanism of the Eye. By Thomas Young, *M.D.F.R.S.* Read Nov. 27, 1800. [*Phil. Trans.* 1801, *p.* 23.]

The copious contents of this lecture relate chiefly to the power possessed by the eye to accommodate itself to the perception of objects at different distances, concerning which a variety of opinions have been entertained.

After some general considerations on the sense of vision, from which it appears that though the extent of the field of perfect vision for each position of the eye be not very great, yet there is reason to believe that its refractive powers are calculated to take in a moderately distinct view of a whole hemisphere: the author, aware how essential it is in an inquiry of so delicate a nature to proceed upon solid, and as far as possible incontrovertible grounds, delivers a set of dioptrical propositions (eight in number), each accompanied by some scholia and corollaries, from which he means to deduce the principal inferences brought forward in the sequel of the lecture.

The nature of these can only be here intimated by their different enunciations, some of which may appear elementary, yet lead to results of a less obvious nature. They are as follows;—1. In all refractions the ratio of the sine of the angle of incidence to the sine of the angle of refraction, is constant. 2. If between two refracting mediums a third medium, terminated by parallel surfaces, be inter-

posed, the whole refraction will remain unchanged. 3. At the vertex of a given triangle to place a given refracting surface, so that the incident and refracting rays may coincide with the two sides of the triangle joined at the vertex. 4. In oblique refractions at spherical surfaces, the line joining the conjugate foci passes through the point where a perpendicular from the centre falls on the line bisecting the chords cut off from the incident and refracted rays. 5. To find the place and magnitude of the image of a small object after refraction at any number of spherical surfaces. 6. To determine the law by which the refraction of a spherical surface must vary, so as to collect parallel rays to a perfect focus. 7. To find the principal focus of a sphere or lens, of which the internal parts are more dense than the external. And lastly, to find the nearer focus of parallel rays falling obliquely on a sphere of variable density. How these various propositions, both problems and theorems, apply to the structure and functions of the eye, will be manifest to those anyways acquainted with investigations of this nature.

As the focal distances of the eye, whether permanent or variable, must be one of the principal data upon which this inquiry is to proceed, an instrument for readily determining these distances could not but be a very essential desideratum. Although due praise be here given to Dr. Portenfield s optometer, invented for that purpose, Dr. Young, thinking it capable of considerable improvements, describes another apparatus of a more simple construction, and much more convenient and accurate in its application. Its principle depends on the circumstance, that when we look at any object through two small holes within the limits of the pupil, if the object be at the point of perfect vision, the image on the retina will be single; but in every other case the image, for reasons previously stated, will become double, and will appear as two lines crossing each other in the point of perfect vision. Thus we see that this point of intersection coincides with that of perfect vision, and by the help of a lens, and of a scale deduced from one of the corollaries of the fourth proposition, we are enabled to determine the focal distance of every eye. The mechanical part of this apparatus must be learnt from the figures which accompany the lecture.

On these principles, and with this instrument, the author proceeds next to investigate the dimensions and refractive powers of the human eye in its quiescent state, and the form and magnitude of the picture which is delineated on the retina. This he has performed chiefly on his own eye; and he has in general grounded his calculations on the supposition of an eye nearly similar to his own. The various expedients he has used for obtaining accurate measurements, is perhaps not the least interesting part of the lecture. Nor will the series of general observations on the structure and functions of the eye, into which the author enters circumstantially, be found of less moment and curiosity. Among these may be noticed the obliquity of the uvea, and of the crystalline lens nearly parallel to the uvea, with respect to the visual ray, whereby a distortion of the focal point is produced

in some eyes, and certain instances of oblique vision may be duly accounted for; also the different refractive powers of the crystalline lens at the centre and near its surface, the former after death being to that of water in the proportion of 21 to 20, and gradually decreasing till at the surface it becomes equal to that of the surrounding medium, thus producing a mean refraction for the whole lens, considered as a body of equal density, in the proportion of 14 to 13 when compared with that of water. We also find here that the whole extent of perfect vision is little more than 10°, or more strictly speaking, that the imperfection begins within a degree or two of the visual axis, and that at the distance of 10° or 15° it becomes nearly stationary, until at a still greater distance vision is wholly extinguished; but that the motion of the eye, at the same time, has a range of about 55° in every direction, so that the field of perfect vision, in succession, is by this motion extended to a circle of 110° diameter. The advantage also of the spherical form of the eye, not only for motion but also for vision, is illustrated by diagrams. These few observations are here inserted not as a just delineation of this important part of the lecture, which cannot be condensed within our limits, but as a few examples of the sort of information the reader may expect to derive from it.

In a following section the author proceeds to inquire how great are the changes which the eye admits, and what degree of alteration in its proportions will be necessary for these changes, on various suppositions;—1. A change in the radius of the cornea. 2. A change in the distance of the crystalline lens from the retina. 3. These two causes acting conjointly; and 4. Some alteration in the figure of the lens itself. A minute inquiry follows next, which of these changes actually takes place in nature: and here a variety of experiments are mentioned, contrived for the purpose of deciding on the truth of each of these suppositions. The object of the first series of these experiments, the results of which were directly inferred from the effects of immerging the eye in water, is to ascertain the curvature of the cornea in all circumstances; and from these results it appears that the cornea is not concerned in the accommodation of the eye. A similar investigation is instituted to inquire whether any alteration in the length of the axis of the eye, which would affect the distance of the lens from the retina, actually takes place in nature. And here, too, the results are, that it is highly improbable that any material change in the length of this axis is ever produced, and that it is almost impossible to conceive by what power such a change could be effected. The opinion of the joint operation of these two causes, which had derived great respectability from the ingenious and elegant manner in which it had been treated by Dr. Olbers of Bremen, and from being the result of the investigation of Mr. Home and the late Mr. Ramsden, is, lastly, shown to be inconsistent with the experiments related in this paper.

We now come to the important section, in which the author inquires into the pretensions of the crystalline lens to the power of altering the focal length of the eye. The grand objection to the

efficacy of a change of figure in the lens, was derived from various instances of persons, who, after they were deprived of that part of the organ, still retained the faculty of accommodation. The result of this inquiry was, contrary to expectation, that in an eye deprived of the crystalline lens, the actual focal distance, as ascertained by the optometer, is totally unchangeable: for the proofs deduced in favour of this assertion, the author acknowledges himself indebted to Mr. Ware, who obligingly introduced him to several of his patients on whom the operation had been performed. Having thus, then, pointed out the inconveniences attending all the other hypotheses, and some imperfections in the experiments adduced in their favour, and having removed the principal objections to the opinion of an internal change of the figure of the lens, the Doctor proceeds to describe some experiments which he conceives come very near to a mathematical demonstration of the existence of such a change, and likewise in a great measure explain its origin and the manner in which it is effected. The results of these experiments are deduced from the different distribution of light in the image of a lucid point on the retina, according to the different states of the eye; and inferring thence, on the mathematical principles above laid down, what form of the lens will account for those different impressions. It is here acknowledged that the mere action of the external coats of the lens, does not, as was stated by the author eight years ago, afford a satisfactory explanation of the phænomenon. It seems, however, manifest, that changes of figure take place in the lens, which can be produced by no external cause; and this seems to establish the muscularity of the lens, long since suggested by Dr. Pemberton, Albinus, and others. The words of the author on this subject are, "If we compare the central parts of each surface of the lens to the belly of a muscle, there is no difficulty in conceiving their thickness to be immediately" (or spontaneously) "increased, and to produce an immediate elongation of the axis, and an increase of the central curvature; while the lateral parts cooperate according to their distances from the centre, and in different individuals in somewhat different proportions." And an intimation is here added, that it would be worthy of inquiry, whether the state of contraction may not also immediately add to the refractive powers of the lens.

In the last section we find some anatomical illustrations of the construction and capacity of the organs of various classes of animals for the functions attributed to them. The human lens is not only ascertained to be of a radiated structure, but, on close inspection, the number of radiations is found to be ten on each side. The greatest pains were taken to trace nerves into the lens, but as yet without success. The author, however, declares his conviction of their existence, and of the precipitancy of those who have absolutely denied them; and adduces some observations in favour of this assertion. He next describes a zone, or gland, surrounding the margin of the crystalline, which he has observed in many animals, and which, from some phænomena of vision, he also infers in the human eye.

Some observations are lastly added concerning the nature and situation of the ciliary processes in various animals; also on the nature of the marsupium nigrum of birds, and the horseshoe-like appearance in the choroid of fishes; both which have improperly been termed muscular,—the former being a mere duplicature of a membrane which may be unfolded; and in the latter the whole mass being evidently of an uniform texture, the fibrous appearance which has misled some former observers being the effect of transverse fissures, or cracks, which may easily be mistaken for filaments.

The lecture concludes with a few observations on the bony scales of the eyes of birds, to which the author denies any concern in changing the focus of the eye; and on a cavity observable in the eyes of some insects which has been supposed to be in some measure subservient to this purpose.

On the necessary Truth of certain Conclusions obtained by Means of Imaginary Quantities. By Robert Woodhouse, *A.M. Fellow of Caius College. Communicated by the Rev.* S. Vince, *A.M. Plumian Professor of Astronomy in the University of Cambridge.* Read January 8, 1801. [*Phil. Trans.* 1801, *p.* 89.]

The object of this paper is to show, that we may be assured of the justness and accuracy of conclusions obtained by means of imaginary quantities, without verifying such conclusions by separate investigations, or without inferring their truth from analogy. In the first part the author premises at some length certain arguments, to show that the operations with impossible quantities must have a logic equally strict and certain with the logic that appertains to real quantities, and that the aid obtained by these quantities would be perfectly useless if such conclusions rested only on the frail basis of analogy.

The author proceeds next to show that operations with imaginary quantities are by no means mechanical, but that they are conducted according to the rules of strict and rigorous logic; and that, although strictly speaking no proposition concerning them can be true or false, yet, after the demonstrations of certain formulæ for real quantities, demonstrations with impossible quantities may be legitimately and logically conducted. The series, for instance, for the development of an exponential, when the exponent is an impossible quantity, can never, independently of certain arbitrary assumptions, be duly established; and yet, when the exponent is the sign of a real quantity, the formula for the development may be rigorously proved. With regard to demonstration, it is shown, as in the case of real quantities, it actually proceeds by a series of transformation, each proved to be the same as the foregoing, not by any arguments grounded on the properties of real quantities, but by reference to the forms certain abridged symbols are made to represent, and to the nature of the operations directed to be performed with certain signs.

After thus establishing the principle by which operations with imaginary characters are regulated, the author shows its efficacy and

the use of imaginary characters in the summation of series, proceeding according to the powers of the series and cosines of arcs in arithmetical progression. He likewise shows, that according to his mode of explanation, certain ambiguous expressions that occur in analysis are perfectly intelligible, and that in the controversy concerning the logarithms of negative quantities, carried on formerly between Leibnitz, Bernoulli, Euler, and Dalembert, all paradox and ambiguity may be made to disappear, by referring to the origin and real import of the impossible exponential quantities.

Although the principal object of this paper is to vindicate the indubitable justness of the operation conducted with imaginary characters, yet in the latter part some arguments are likewise offered in favour of the commodiousness of imaginary expressions for facilitating calculations. And, lastly, it is contended, that in the present state of analysis, these expressions are particularly useful in deducing certain conclusions, which without their aid could not be obtained without much difficulty.

On the Production of Artificial Cold by Means of Muriate of Lime. By Mr. Richard Walker. *Communicated by* Henry Cavendish, *Esq. F.R.S.* Read January 22, 1801. [*Phil. Trans.* 1801, *p.* 120.]

Mr. Walker, since his late communications to the Society on the best means of producing artificial cold, received intelligence that Mr. Lowitz, Professor of Chemistry at Petersburgh, had made some experiments, in which a neutral salt different from those he had himself used, and which is but little known or attended to, produced effects which exceeded his expectations. The salt is the muriated lime; which, mixed with snow in the proportion of about 3 to 2, at the temperature of $+27^{\circ}$, produced a refrigeration which sunk the thermometer to -55°; and with this mixture the Professor in one experiment froze no less than 35lbs of quicksilver.

Mr. Walker repeated the experiment with success; but finding that it can only be made during a freezing atmosphere, he resolved to try the effect of this salt, reduced to such a strength by evaporation as to endure being kept in a solid state throughout the year. After describing the expedients used for this purpose, he enumerates the results of two sets of experiments; the first made with the muriated lime prepared so as to be used in winter only, that is, of the specific strength of 1·450; and the other made with the salt prepared so as to be kept for use at any time, the strength of which was 1·490. The apparatus here used (though somewhat improved) is not unlike that described in Mr. Walker's former communication, nor is the process materially different.

The paper concludes with a general view of the different frigorific mixtures:—1st, those composed of chemical substances with ice; and 2nd, those in which the use of ice is dispensed with. In a postscript the author suggests a method of obtaining transparent ice, fit for optical purposes, which is effected merely by immersing a vessel

containing a frigorific mixture in water: by this means he has frequently obtained a pellucid coating of ice on the outside of the vessels, of considerable thickness, and, by adapting the form of the vessel, of any figure that might be required.

Account of a monstrous Lamb. In a Letter from Mr. Anthony Carlisle *to the Right Honourable Sir* Joseph Banks, *Bart. K.B. P.R.S.* Read January 29, 1801. [*Phil. Trans.* 1801, *p.* 139.]

The head of this animal, or rather fœtus, for it was not born alive, was disproportionately small, and had no resemblance to the natural form except in the external ears, which were contiguous, and placed on the front part of the head. Between them was an opening, which proved to be the common passage to both the œsophagus and the trachea. All the organs which are usually found on the face were here wanting; there being neither eyes, nose, nor any of the apparatus belonging to the mouth: the cranium was formed into a hard bone, bearing a near resemblance to the head of a tortoise, and about the size of a plover's egg.

On dissecting this singular production, it was found that the whole cerebrum and all its nerves were wanting. It is hence inferred that the formation and growth of animals in the uterus are independent of any influence from those parts of the brain which properly belong to sensation. The author regrets that this animal did not live to show the phænomena of volition directed to its limbs and other parts, without that intelligence from the organs of the senses which regulate the actions of perfect animals. A careful observance of such circumstances, he thinks, might lead to discoveries of the greatest importance in that part of physiology which is still enveloped in much obscurity.

An Anatomical Description of a male Rhinoceros. By Mr. H. Leigh Thomas, *Surgeon. Communicated by* George Fordyce, *M.D. F.R.S.* Read January 29, 1801. [*Phil. Trans.* 1801, *p.* 145.]

An opportunity having lately offered of examining a living rhinoceros, and of dissecting it after death, Mr. Thomas availed himself of the favourable incident; and in this paper affords us all the information he could gather concerning that curious animal.

Dr. James Parsons having, upwards of fifty years ago, laid before the Society some account of the external parts of a rhinoceros, a recapitulation is here given of what is contained in that paper; but on the other hand, the description of the internal parts, and of some of the organs, is the more ample, and, together with some observations on its habits, will probably prove equally satisfactory to the anatomist and the physiologist.

Without entering into the technical part of this description, we shall only notice here certain peculiarities concerning the eye, in

which a structure has been observed, of which no other animal is as yet known to partake.

In cutting through the eye, four processes were met with, arising by distinct tendons from the internal or posterior portion of the sclerotic, which, passing forward, gradually became broader, and insensibly lost themselves in and formed a part of the choroid. These processes had a muscular appearance. The ciliary processes were affixed to the crystalline lens, which was nearly spherical, with the anterior surface somewhat flattened.

Concerning the use of this structure, especially of the processes, the author observes, that as the natural unwieldiness of this animal probably unfits it for quickly directing its sight to objects which for its own preservation it ought to be able to examine easily and minutely, nature seems to have supplied it with an apparatus calculated to remedy this imperfection. The change in the eye, which adapts it for distinct vision at different distances, the author ascribes in a great measure to the four above-mentioned processes, which upon contracting will shorten the axis of the eye, and produce the desired effect: for near objects, it is observed that this animal has the eyes placed much nearer the mouth than any other, whence, without any adaptation of the eye, it is capable of performing the most essential of its functions, viz. that of examining the food necessary for its subsistence.

Demonstration of a Theorem, by which such Portions of the Solidity of a Sphere are assigned as admit an algebraic Expression. By Robert Woodhouse, *A.M. Fellow of Caius College, Cambridge. Communicated by* Joseph Planta, *Esq. Sec.R.S.* Read February 12, 1801. [*Phil. Trans.* 1801, *p.* 153.]

In the second volume of the Memoirs of the National Institute, M. Bossut announces a theorem relative to the solidity of a sphere similar to Viviani's, by which quadrable portions of a hemispherical vault are assigned. M. Bossut withholds the analysis that led him to his result, but mentions that it involves an integration much more complicated than that which occurs in Viviani's problem. In the present paper, Mr. Woodhouse furnishes the analysis that leads to the result announced in Bossut's theorem, and, by a transformation of the co-ordinates of the sphere, arrives at a differential expression, the integration of which does not appear more complicated than that employed in the solution of Viviani's problem.

Account of the Discovery of Silver in Herland Copper Mine. By John Hawkins, *Esq. Communicated by the Right Hon. Sir* Joseph Banks, *Bart. K.B. P.R.S.* Read February 12, 1801. [*Phil. Trans.* 1801, *p.* 159.]

This ore was found in the Herland mine, about six miles from St. Michael's Mount in Cornwall. It consists chiefly of lodes of

copper, of which several run nearly in a parallel direction from east to west.

The peculiarity here is, that there are several cross courses which run north and south, the greatest part of which contain no metal: these meeting with the lodes of copper ore, interrupt their continuity, or, as is expressed by the miners, heave them out of their direction, so that at the place of intersection the copper lodes seem to have been forced aside eighteen or twenty inches. One of these cross courses has of late been discovered to yield silver in no inconsiderable quantity; but with this particular circumstance, that at and near the place of intersection the ores both of silver and copper are much less productive than at some distance from it. The silver ore consists of a mixture of galena, native bismuth, gray cobalt, vitreous silver, and native silver chiefly in a capillary form.

Account of an Elephant's Tusk, in which the Iron Head of a Spear was found imbedded. By Mr. Charles Combe, *of Exeter College, Oxford. In a Letter to the Right Hon. Sir* Joseph Banks, *Bart. K.B. P.R.S.* Read February 19, 1801. [*Phil. Trans.* 1801, *p.* 165.]

This tusk, which weighed fifty pounds, and measured about six feet in length, is supposed to have been imported from Africa, and was purchased at Liverpool by a manufacturer of Birmingham. On shaking it, a rattling noise was heard near the middle part, which, on cutting the tooth transversely, was found to be occasioned by an iron spear-head, about six inches and a half long, which lay in the longitudinal direction of the tooth, with the point foremost, and was considerably corroded. It is conjectured that the spear had entered at the basis of the trunk, between the interior angle of the eye and the proboscis, the cavity of the tusk being placed immediately beneath this part. From the quantity of bony matter that had been formed round this extraneous body, it is inferred that the animal must have lived a considerable time after it had received the wound. —The spear-head and the part of the tooth in which it had been imbedded were exhibited to the Society at the close of the Meeting.

Description of the Arseniates of Copper, and of Iron, from the County of Cornwall. By the Count de Bournon. *Communicated by the Right Hon. Sir* Joseph Banks, *Bart. K.B. P.R.S.* Read February 19, 1801. [*Phil. Trans.* 1801, *p.* 169.]

In the mine called *Huel Gorland*, in the parish of Gwennap in Cornwall, has lately been discovered a species of ore, consisting of different combinations of the arsenic acid with copper and iron, which, though some mention have been made of it by German writers as having been found in Silesia, appears yet, from the great abundance afforded by the above-mentioned mine, to be almost peculiar to this country. After an historical account of the little that has hitherto been known concerning this substance, Count Bournon

proceeds to describe the external appearance of the specimens he has had opportunities to examine; and from these characters, together with an accurate analysis of the different sorts made by Mr. Chenevix, he deduces the following classification of this new mineral.

The principal distinction is in arseniates of copper and of iron. Of the former there are four species :—

1. Arseniates of copper, in the form of an obtuse octaedron. This is said to be the most simple, and appears to be the original form of an ore. The minute descriptions of the crystals are in this, as in all other species, illustrated by delineations. This sort is very light, its average specific gravity being 28·819. Its hardness is inconsiderable, and its transparency seldom perfect. Its colour is either a beautiful deep blue or a fine grass-green, and sometimes white with a slight blue cast. According to the analysis, this variety of colours depends chiefly on the quantity of water which enters into the crystallization.

2. Arseniate of copper in hexaedral laminæ with inclined edges. The colour of this ore is a fine deep emerald green, and sometimes a light green. Two of the six sides have so great a lustre that they often assume the appearance of metallic foils. Its specific gravity is 25·488. It is less hard than the preceding species. Its thin crystals are transparent, and it frequently decrepitates in fire.

3. Arseniate of copper in the form of acute octaedrons. Its colour is a brown or bottle-green, the surface often reflecting a golden tint. Its specific gravity is 42·809. It is sufficiently hard to scratch fluor spar, but not glass. Its transparency is generally very great. These general characters are by no means permanent. Its crystals are not always determined, nor does it at all times exhibit a similarity of colours. These differences of form and aspect have induced the Count to distinguish five varieties, which are denominated as follows: —1. Capillary of a determinate form; 2. Capillary of an indeterminate form; 3. In crystals perfectly regular in one part of their lengths, and fibrous at their extremity; 4. Amianthiform, consisting of very delicate flexible fibres; and 5. Hematitiform, not unlike the tin ore known by the name Wood-tin.

4. Arseniate of copper in the form of a triedral prism.—As the crystals of this species are seldom sufficiently insulated to be distinctly perceivable, and are in general so small as to escape the naked eye, all their various forms are here more particularly described, and also their progressive change of figure as they deviate from their primitive configuration. Its specific gravity is 42·809. Its hardness is not so great as that of the preceding species, as it does not easily scratch calcareous spar. Its crystals are often transparent, and of a very beautiful blueish-green colour; but their surface easily decomposes, and the crystals then become black and perfectly opaque.

The second class, being the arseniates of iron, is distinguished into two species :—

1. The arseniates of iron, properly so called.—This ore crystallizes in perfect cubes, sometimes, though rarely, a little flattened. Its

specific gravity is 30·000: its hardness just sufficient to scratch calcareous spar. Its crystals, which are tolerably transparent, are of a dark green colour, with sometimes a brownish tinge; and when a decomposition takes place, the crystals pass into the state of a pulverescent oxide of a fine reddish yellow colour.

The 2nd species is denominated cupro-martial arseniate. Its crystals are of an uncommon brilliancy, and perfectly transparent: their form is a rhomboidal tetraedral prism. Its specific gravity is 34·003: its hardness rather greater than that of the arseniate of iron; the colour, a very faint sky-blue, sometimes a little deeper. Hitherto this ore has not been met with in any form but that of a perfect crystal.

Analysis of the Arseniates of Copper, and of Iron, described in the preceding Paper; likewise an Analysis of the red octaedral Copper Ore of Cornwall; with Remarks on some particular Modes of Analysis. By Richard Chenevix, *Esq. M.R.I.A. Communicated by the Right Hon. Sir* Joseph Banks, *Bart. K.B. P.R.S.* Read March 5, 1801. [*Phil. Trans.* 1801, *p.* 193.]

After having perused the accurate detail of the external characters and crystalline varieties by which Count de Bournon, in a paper he lately communicated to the Society, identified the several species of the ores mentioned in the title, the mineralogist will surely be gratified to find in this, which may be considered as a continuation of the Count's treatise, an elaborate analytical investigation, whereby not only the above-mentioned classification is fully confirmed, but also some progress is made in the theory of the docimastic art. After pointing out the principal imperfection of this art, and in what manner more precision is likely to be obtained, both as to the processes and the terms used in describing them, Mr. Chenevix enters into a full detail of the mode in which he proceeded in his inquiry, and of the many precautions he used in order to obviate every possible deception. The results, he repeatedly declares, have been to him singularly satisfactory, as they fully evince that great confidence can be placed in the crystallographical arrangement, and that in fact the evidence obtained by the two modes reciprocally contribute to confirm the inferences derived from each.

These results, as far as they relate to the arseniates of copper, are briefly these:—

The natural arseniate of copper exists in three different states of combination; the first containing 14, the second 21, and the third 28 per cwt. of the arsenic acid.

Each of these may contain different proportions of water, either as constituting a hydrate, or as water of crystallization.

Upon losing their water they generally pass from a blue to a pale green colour, and finally to brown.

One species only, being the first of the Count's classification, can be considered as a real arseniate of copper; the others, from the

quantity and combination of the water they contain, being more properly arseniates of hydrate of copper.

This first species is not to be put on the same footing with the others; since, by admitting a due proportion of water, it would, by calculation, be reduced to a lower class than that which it really occupies.

Lastly, the proportion of acid in each of the species except No. 2. is here assigned. And as to this last-mentioned species, it is observed, that it is to be considered as a particular variety, consisting of a much greater proportion of oxide, with a less quantity of water, combined with nearly the same proportion of arsenic acid.

After having carefully examined the natural arseniates, Mr. Chenevix paid some attention to a few artificial ones, being precipitates from nitrate of copper, by an arseniate of ammonia. And here he found very different proportions of the ingredients, the arsenic acid in one species being no less than 40 per cent. Hence we gather, that only two simple ingredients, combined in four different proportions, produce no less than eleven different species or combinations, which are now determined both by external and chemical characters.

Speaking next of the arseniates of iron, Mr. Chenevix observes, that they have but lately been distinguished from those of copper. One species, in fact, contains a sufficient proportion of this last metal to merit the name of *cupreous arseniate of iron*. This proportion was 27·5 of iron to 22·5 of copper, both in the oxide state; the arsenic acid amounting to 33·5, whilst 12 of water and 3 of silica made up within $1\frac{1}{2}$ the 100 parts on which the experiment was tried. The proportions of what is properly called the *arseniate of iron* were 45 oxide of iron, 9 oxide of copper, 31 arsenic acid, 10 water, and 4 silica. Upon this ore are often seen certain crystals of a cubic form and of a deep brownish red, which, according to Count de Bournon, are in a state of decomposition. These were found to contain but little acid or water, probably owing to their decomposition.

Artificial arseniates of iron, produced by the decomposition of green and red sulphate of iron by arseniate of ammonia, were next examined. The ingredients of the green arseniate were found to be, 43 oxide of iron, 38 arsenic acid, and 19 water; and of the red arseniate, 36·5 oxide of iron, 41·5 arsenic acid, and 20 water.

Observing in the course of these experiments a great variety of appearances assumed by the combinations of iron with salts, the oxygen, and other ingredients, the author enters into a curious inquiry on this subject; from which he deduces, in particular, the great variety of colours exhibited by that metal in divers stones or fossils, in which that variety, he infers, is derived from the different degrees of oxygenation of the iron.

In a third section the author enters into an analysis of a red octaedral copper ore found in Cornwall, of which he had occasion to examine several specimens in the preceding investigation. After describing several fruitless attempts, chiefly by means of acids, to

decompose this ore, he had recourse to a process to which he was led by some instances he had before observed, in which a mixture of two compounds of the same ingredients, but in different proportions, remained insoluble, while a third substance seemed to operate upon at least one of these two compounds, and to produce the decomposition that was aimed at. The following is the manner in which he explains this operation:—When a metallic oxide A, for instance, containing 25 per cent. of oxygen, is in contact with the metallic oxide B, containing 10 per cent., they will each remain quiescent in their respective states: but if a solvent C, for which the substance B has no affinity at 10 per cent. of oxygen, but a powerful one at 15 or 20 per cent., comes to be added, then may the oxide A lend a part of its oxygen to B, in order to enable it to combine with the solvent C. Thus when phosphoric acid had dissolved all it could of the pulverized ore, its oxygen in the part undissolved was concentrated, as it were, to the amount of about 20 per cent.; and all that which could not be dissolved, would, through a twofold affinity of copper for oxygen to the amount of 20 per cent., and of phosphoric acid for the oxide of copper of that degree of oxidation, yield up its entire share of oxygen, to favour the combinations which took place in the new order, the only one which could exist among the substances now present.

From the variety of experiments founded upon this reasoning, it has been gathered, that the copper in this ore contains much less oxygen than has ever been suspected in any oxide of copper; and that, from the quantity of the copper which was precipitated in the metallic state by iron, it appears to be combined in the proportion of only 11½ per cent., the rest being pure copper,—a state of metallic concentration of which no instance has as yet been observed in nature.

It is lastly suggested, that, considering not only the great purity of this ore, but also the singular facility with which this useful metal may be extracted from it, it will be found much superior to every copper ore hitherto discovered. It contains no iron and no sulphur; the absence of which latter is a peculiar advantage. It is hence strongly recommended to the proprietors of mines to be particularly attentive to this ore, which is said not to be uncommon in some parts of Cornwall, whereby they are likely not only to further their private advantage, but may also materially contribute to promote the public utility.

A Historical and Anatomical Description of a doubtful amphibious Animal of Germany, called, by Laurenti, *Proteus Anguinus. By* Charles Schreibers, *M.D. of Vienna. Communicated by the Right Hon. Sir* Joseph Banks, *Bart. K.B. P.R.S.* Read March 26, 1801. [*Phil. Trans.* 1801, *p.* 241.]

This singular production has hitherto been found only in a small lake in Carniola, called Sitticher See, and has never yet been met

with in other large lakes of the neighbourhood, although these be known to communicate with the former by subterraneous channels. The specimens which have as yet reached either the public or private collections are so few, that all the descriptions hitherto given by Laurenti, Scopoli, Herman, Schneider, and Gmelin, have been found equally defective and erroneous, especially as to the anatomical construction, which, indeed, those able naturalists have scarcely had opportunities of investigating. This defect probably gave rise to a difference of opinion concerning the class to which this animal is to be annumerated; some considering it as a species arrived at its degree of perfection, while others maintain that it is the larva of some kind of lizard hitherto unknown.

The principal object of this paper is to offer so circumstantial a description of the different parts of this animal as to enable physiologists to determine the point hitherto undecided. The specimen from which this description was taken measured about thirteen inches in length, and one inch in diameter; the fore part of the head was flat and narrow, somewhat resembling the bill of a duck: the upper lip projected considerably beyond the lower one. No external traces of nostrils, ears or eyes could be discovered. Of the latter, however, some indications are thought to have been perceived on a living specimen. On each side of the occiput was an opening, like those of fishes; and over them certain branchial appendages, similar to those in tadpoles and other larvæ of amphibious animals; whence probably arose the difference of opinions concerning the nature of this animal. From the description here given, we are to infer, that the construction of these parts, when carefully examined, differs materially from those as well of fishes as of tadpoles or other larvæ.

The body is round, equally thick throughout between the fore and hind feet: the fore feet are about one inch long, each having three toes without nails, the hind feet about two lines shorter with only two toes: behind the latter the body grows narrower, and terminates in the tail, which is compressed on the sides, and ends nearly in a point. The skin is coriaceous; but looking at it with a magnifier, it exhibits a number of minute glands underneath the epidermis, similar to those in water-lizards, &c. Its colour when alive is a light red; but when kept a while in spirits, it becomes of a dusky brown.—A detailed account is also given of the muscular fibres under the skin.

Upon opening the body by a longitudinal section, the whole cavity was found almost filled by the liver, extending from the thorax down to the pelvis, so as to cover the greatest part of the other viscera. The heart consists of a single ventricle, and an auricle as large as the ventricle. The situations, dimensions, and structure of these, as well as of the stomach, intestines, gall-bladder, spleen, kidneys, pancreas, &c., are minutely described: and as it was found to have something particular in its formation, the author dwells somewhat more at large on the air-bladder, or pneumatic apparatus, which he met with in the thorax, immediately below the heart. This he found to be a simple bag, without any cellular structure, as in the

respiratory organs or lungs of other amphibious animals; but a similar viscus he has hitherto sought for in vain in the larvæ of water lizards or other animals of that description.

Concerning the habits of this singular animal, we learn from a friend of the author, who resides near the lake where it is found, and who had the good fortune to keep one of them alive during several days, that it seemed at all times very torpid; that though it would occasionally swim with the help of its broad tail, it was in general motionless at the bottom of the water. Sometimes it rose to the surface, stretched its head out of the water, seemed to take in air, but immediately returned to the bottom. It crept by means of its feet both at the bottom and on the side of the vessel, but so slowly that the circumstance may be thought characteristic of the animal. Sometimes, putting its head out of the water, it produced a hissing noise, louder than could have been expected from so small an animal.

The author, lastly, compares this singular production with the *Siren lacertina* of Linnæus, which has since been classed with the fishes under the name of *Muræna Siren*, and finds a considerable analogy between the two: and though he seems unwilling to determine whether the animal he describes be perfect, or only a larva of some unknown species, the facts he has adduced will probably be thought to favour the former of these opinions.

Observations tending to investigate the Nature of the Sun, in order to find the Causes or Symptoms of its variable Emission of Light and Heat; with Remarks on the Use that may possibly be drawn from Solar Observations. By William Herschel, *LL.D. F.R.S.* Read April 16, 1801. [*Phil. Trans.* 1801, *p.* 265.]

The principal object of this paper is to explore the causes or symptoms of the variation we observe in the emission of light and heat from the sun.

Considering the great influence of these agents on most of the concerns of life, it is scarcely necessary to point out the importance of the inquiry: not that any discoveries we may make on the subject will ever enable us to modify their operations, but that, by a due knowledge of them, we may be guided in our own proceedings, in the same manner as we frequently are by the meteorological instruments, on whose combined indications we have been taught to place a certain degree of confidence.

In order to obtain as intimate a knowledge of the sun as that which is required for the purpose here indicated, it is obvious that the first step must be to become well acquainted with all the phænomena that usually appear on its surface: and this accordingly is the subject of the first part of the present paper. Dr. Herschel premises his reasons for substituting a new set of names for those of spots, nuclei, penumbræ, faculæ, and luculi, hitherto used to denote certain appearances on the sun. Those he adopts are, openings, flats, ridges, nodules, crankles, shallows, dimples, and punctures.

The following definitions of these new terms, together with the principal circumstances relating to those appearances, as deduced from a long series of observations, will, it is hoped, sufficiently indicate the contents of this section. Whoever peruses this paper must, however, here recollect that Dr. Herschel has long considered the sun as an opake habitable globe, possessed of an atmosphere in which luminous clouds, ever varying in form and dimensions, are continually floating, and produce the appearances of which the following is an enumeration.

1. *Openings*, or places where the luminous clouds are removed.—When these are large, they have generally flats about them; and the small ones are without flats. They are also frequently attended by ridges and nodules. New and incipient openings frequently break out near former ones; and they often change their figure, run into each other, and turn into shallows, or other appearances of a different description.

2. *Flats*.—These are described as planes depressed below the general or brightest surface of the sun, or places from whence the luminous solar clouds of the upper regions are removed. Their thickness is visible at the edges of the openings: from the various changes they undergo, it is inferred that they are occasioned by some emanation, perhaps an elastic gas, coming out of the openings, which by its propelling motion drives away the luminous clouds from the place where it meets with the least resistance, or which by its nature dissolves them as it comes up to them.

3. *Ridges*, or elevations above the general surface of the luminous clouds of the sun.—These generally accompany openings, and often gather and disperse alternately. They are ascribed to some elastic gas, acting below the luminous clouds, which first lifts them up, and at last forces itself a passage through them by throwing them aside.

4. *Nodules*.—These are small but highly elevated luminous places. They may frequently be ridges fore-shortened, and are probably in all cases produced in the same manner.

5. *Crankles*.—These consist of elevations and depressions, which produce a mottled appearance that often spreads over the whole disk of the sun. They frequently change their shape and situation, and may perhaps be occasioned by the expansion of ridges or nodules.

6. The dark parts of crankles are here called *Shallows*.—The small ones have no openings; but in some larger ones apertures have been perceived, through which the opake part of the sun was discernible. They are thought to be of the same nature as flats, and are perhaps at the same depth below them as the flats are below the general surface of the sun.

7. *Dimples* are small depressions, or indentures, and often contain very small openings. They differ from crankles chiefly in size.

8. Lastly, the low places of dimples are called *Punctures*. These increase sometimes, and become openings, and at other times vanish very rapidly.

Having thus enumerated, according to his new nomenclature, the

phænomena from which he derives his inferences concerning the nature of the sun, Dr. Herschel proceeds next to treat of the *regions of solar clouds*. The point he here principally insists upon, is, that the above-mentioned appearances are wholly incompatible with the hypothesis of the shining matter of the sun being a liquid, or an elastic fluid of an atmospheric nature; since, by the laws of hydrostatics, all the depressions would be instantly filled up, and the elevations would as rapidly subside. The opinion he advances is, that this shining matter exists in the manner of empyreal, luminous, or phosphoric clouds, residing in the higher regions of the solar atmosphere. Of these he assumes two different regions, or a double stratum of clouds, whereof the lower, viz. that which is nearest the sun, consists of clouds less bright than those of the upper stratum. The lower clouds are also more closely connected, while the upper ones are chiefly detached from each other, and permit us everywhere to see through them.

A number of additional observations are here added; from which it is inferred, that the inferior clouds are opake, and probably not unlike those of our planet; and that their light is only the uniform reflection of the surrounding superior, self-luminous region. These lower clouds, it is thought, compose what the Doctor calls *flats;* and by a contrivance here described, he demonstrates, that the quantity of reflected light they transmit to us is to that of the superior and self-luminous clouds in the proportion of 469 to 1000.

By the same process he proves also that, adhering to the same proportion, the quantity of light reflected by the solid body of the sun at the openings is represented by a number no greater than seven. Speaking of the planetary clouds, it is shown of what eminent service they must be to the whole solar system; since, by their means, nearly one half more light is transmitted to us from the sun than we should receive from the self-luminous stratum alone.

In a section *on the solar atmosphere*, after showing that its existence cannot be denied,—since the clouds could not be kept suspended in the manner in which they are without an elastic atmospherical fluid to bear them up,—reasons are assigned why this atmosphere must be of a greater extent, of considerable density, transparent, and, like ours, subject to agitations by winds and other disturbing causes.

From these various observations and inferences is next derived a *theoretical explanation of the solar phænomena;* wherein the manner is described in which all the above-mentioned appearances on the surface of the sun are likely to be generated: after which follows an enumeration of the *signs from which we may infer a deficiency or abundance of luminous matter in the sun*. The former of these are a deficiency of empyreal clouds, of openings, and of ridges, nodules, and all that may be considered as prominences; whereas the opposite appearances are indications of increasing light and heat. The Doctor now does not scruple to assert that openings with great flats, ridges, nodules, and crankles, may induce us to expect a copious emission of heat, and therefore mild seasons; and that, on the con-

trary, punctures, dimples, and a poor appearance of the luminous clouds, the absence of ridges, nodules, large openings, and flats, denote a spare emission of heat, and may induce us to expect severe seasons.

Pursuing this last idea, Dr. Herschel subjoins, at the end of his paper, a comparative view of the best accounts that are to be met with of the appearances of the sun at particular periods as far back as the middle of the seventeenth century, with the state of the seasons during the same periods. Of the latter, the best information could *only* be gathered from the state of vegetation, particularly of corn, of the price of which registers have been kept many years back: and though this price be by no means an accurate criterion of the quantity of corn produced, yet it is recurred to as the least objectionable that could be obtained. The result of this review actually leads to the conclusion, that the price of wheat has constantly risen during the time the sun has been without spots; and that it has always fallen when those spots began to re-appear.

The Doctor seems aware of some fallacy in this mode of argumentation; but he adds some hints by which several of the objections might, he thinks, be obviated.

Observations on the Structure, and Mode of Growth, of the grinding Teeth of the Wild Boar, and Animal incognitum. By Everard Home, *Esq. F.R.S.* Read May 7, 1801. [*Phil. Trans.* 1801, *p.* 319.]

The author on a former occasion laid before the Society an account of certain peculiarities in the growth of the grinding teeth of the Sus æthiopicus, and pointed out the *similarity of their structure* to that of the elephant. Having since discovered that a like resemblance extends also to the dentition of the wild boar, though in a less degree, and at a later period of life, he is pleased to communicate to the Society, in his present paper, some further remarks on this curious subject.

We here learn, that in the species of the Sus, the first or temporary grinders are sixteen in number; viz. four in each side of the upper, and as many in the under jaw; that these are shed in the usual manner; and that their places are supplied by larger teeth, rising from the substance of the jaw, immediately under the old ones; that before these first teeth are shed, one of the more permanent grinders is formed in the posterior part of each jaw, which, although it be in its place with the first set, is yet to be considered as belonging to the second; that besides these five teeth, the rudiments of a sixth are formed in each jaw, which afterwards grows larger than the preceding ones, the jaw increasing in size, so as to make room for this as the posterior grinder; that this tooth, when perfect, is double the size of the other grinders, its masticating surface having eight fangs, so that it very much resembles two large grinding teeth incorporated into one; that, in time, the rudiments of a seventh tooth

appear in each jaw, but that the further progress of these could not be observed, none of the specimens the author has had opportunities of inspecting, appearing to be more than seven years old.

Mr. Home proceeds next to observe, that the elephant, the Sus æthiopicus, and the wild boar, are the only recent animals in which he has hitherto met with so extensive a masticating surface of the grinding teeth; the human species only excepted, in which the mode of dentition is somewhat upon the same principle as that of the wild boar, with this difference, that the hindmost teeth, called, from the late period of life at which they cut the gum, Dentes Sapientiæ, do not exceed the others in size, and have often not sufficient room in the jaw to come into their regular place. A conjecture is hence derived, that when the period of man's life was longer than it is at present, the growth of the posterior part of the jaw was continued for a greater length of time, so as not only to make room for the present, but perhaps also to admit of a succession of a still greater number of additional grinders.

Upon comparing the grinders of the boar with the large fossil teeth found on the banks of the Ohio, they were found so much alike, both in their external appearance and internal structure, as to render it more than probable that they are teeth of the same kind, only differing in size. Not so, however, those of the fossil skeleton some time since found in South America, and described by M. Cuvier. These were found so unlike those of the boar, or the above-mentioned incognitum, as to leave no doubt of its being an animal of a different genus.

From the progressive mode of dentition above described, it is inferred, that the animals to which it appears to be peculiar, have by nature been intended for great longevity. This we know to be the case in the elephant: and though opportunities have not yet offered for ascertaining the term of life of the wild boar, some quotations from ancient authors are here adduced, which indicate that boars of enormous size have at different times existed; whence the probability is inferred that their bulk must have been the growth of many years.

Account of some Experiments on the Ascent of the Sap in Trees. In a Letter from Thomas Andrew Knight, *Esq. to the Right Hon. Sir* Joseph Banks, *Bart. K.B. P.R.S.* Read May 14, 1801. [*Phil. Trans.* 1801, *p.* 333.]

The author prefaces his paper by declaring that the cause of the ascent of the sap in trees appearing to him not to have been as yet satisfactorily accounted for, he resolved to enter on an experimental inquiry on the subject; and that having met with some facts of which he had found no mention in any author, he flattered himself an account of them might not be unacceptable to the Society.

The first experiments were made with a view to determine whether the sap does actually, as has been thought by some, ascend along

the bark. For this purpose, circular incisions were made round a number of young healthy trees, and the bark removed for the space of about half an inch, or more, of the whole circumference. This, it was found, by no means impeded the growth of the upper part of the tree; but, on the other hand, the part of the stem below the incision scarcely grew at all, and in time even seemed to wither. From the whole of this investigation it seems probable, that the current of sap which adds the annual layers of wood to the stem, so far from ascending, actually descends from the young branches and leaves through the bark. The branches and leaves which supply this fluid became hence the next objects of the inquiry.

The conjecture just now mentioned was here confirmed by the circumstance, that when a branch or leaf was left between two circular incisions, it continued to receive its nourishment as usual; and the bark under it gave evident marks of increasing vegetation, while that above was not only stationary, but seemed even to decay.

It became now necessary to investigate by what channels leaves receive their nourishment. Some annual shoots were cut from trees, and placed in a coloured infusion. Although this fluid, it was observed, certainly rose into the leaves, yet neither the bark nor the medulla was sensibly tinged by it; but in the centre of the stalks of the leaves were found several bundles of tubes which had been manifestly coloured, and must hence have been the channels of communication. These tubes were surrounded by others, which, being traced downwards, were found to enter the inner bark, and by no means to communicate with the tubes of the wood: these being colourless, it may reasonably be concluded that they convey a different fluid from that which ascends into the leaf.

To the former, or internal tubes, which had not yet been distinguished by any name, the author thinks fit to assign the appellation of Central Vessels. He then mentions certain spiral tubes which are everywhere appendent to these vessels, and seem to proceed from the sides of the medulla to the leaf-stalk. Particular attention is then paid to the action of the medulla. By extracting parts of it out of the stems of trees, so as completely to interrupt its continuity, it was proved beyond a doubt that it is nowise necessary for the progression of the sap, the tree growing equally, whether this marrow be or be not continued.

The next set of experiments relates to the fructification: and here central tubes were likewise found in the fruit-stalks, which, there is reason to think, are the nourishing ducts of those productions. Many curious circumstances are here mentioned concerning the internal organization and mode of nutrition of certain fruits, such as apples and pears, for which, as well as for several observations on the error of those who have ascribed the ascent of the sap to capillary tubes, or to the sole agency of heat, we must refer to the paper, in order to hasten to the part in which the author points out an agent to which he thinks the mechanical propelling force required may be reasonably ascribed.

In all kinds of wood, he says, there are two sorts of grain,—the false or bastard, and the true or silver grain. The former consists of the concentric circles which mark the annual increase of the tree; and the latter is composed of thin laminæ, diverging in every direction from the medulla to the bark, with different degrees of adhesion to each other at different seasons, and lying between and pressing on the sap-vessels of the alburnum.

If these laminæ are expansible under various changes of temperature, or from any other cause arising from the powers of vegetable life, our author conceives that they are as well placed as is possible to propel the sap to the extremities of the branches. That they are affected by the changes of temperature in the air is proved by the effects of these changes on them even after the tree is dead, as in the instance of boards, which warp more or less, according to the direction of this grain: and other instances are given of the effects of solar heat on different parts of plants, which materially favour this assertion.

The general conclusions derived from these experiments are, That the tubes of the alburnum, acted upon by the agency of the silver grain, are in fact the channels which, extending from the extremities of the roots to the points of the annual shoots, convey the nutricious juices to the base of the buds, and in the soft and succulent part of the annual shoot, where the alburnum with the silver grain ceases to act, and where commences the action of the central vessels, with their appendages the spiral tubes;—that having through these reached the end of the leaves, the sap undergoes a change, perhaps from the action of the atmosphere, and is then brought back again through the external vessels of the leaf-stalks to the bark, which conveys it to every part of the tree, and ultimately contributes to its growth.

In speaking of the use of the medulla, the author assigns his reasons for considering it as a reservoir of moisture, which it occasionally imparts to the leaves and fruit through the central vessels, and which these organs must often stand in need of, as they cannot, like animals, resort to the brook or shade. The heart or coloured wood of the trees he considers as the bones in the animal œconomy, being intended to support them against the effects of winds and other destructive agents; and, accordingly, it is not found in roots or tender shoots, but is only formed when the vegetable has acquired a bulk which renders such a structure necessary.

Additional Observations tending to investigate the Symptoms of the variable Emission of the Light and Heat of the Sun; with Trials to set aside darkening Glasses, by transmitting the Solar Rays through Liquids; and a few Remarks to remove Objections that might be made against some of the Arguments contained in the former Paper. By William Herschel, *LL.D. F.R.S.* Read May 14, 1801. [*Phil. Trans.* 1801, *p.* 354.]

This may be considered as a supplement to Dr. Herschel's paper on the nature of the sun, lately read to the Society, and consists

chiefly of a continuation of his observations on the appearances of that body from the 2nd of March to the 3rd of May last.

Conceiving that there might be some advantage in getting rid of the darkening glasses in viewing the sun, he was led to substitute for them various liquors, such as spirits of wine, port wine, ink diluted with water, a solution of green vitriol with a small proportion of tincture of galls, and even plain water; which latter he found keeps off the heat so effectually, that the brightest sun may be viewed some time through it without any inconvenience.

Through diluted ink, the image of the sun appeared as white as snow; and when the liquor was still more diluted, the sun was of a purple hue, while the objects on its surface continued as distinct as when seen through any other medium. From these observations the author infers that the continuance of the symptoms which in his former paper he considered as favourable to the copious emission of light and heat from the sun, are sufficiently verified, and that by comparing these phænomena with the corresponding mildness of the season, his arguments respecting the connexion between them and the temperature of our atmosphere acquire no small degree of probability.

Being well aware that the price of wheat which he adopted in his former paper as a criterion of the seasons is liable to some objections, the author desires here to be understood, that his intention was merely to compare the astronomical fact of the variable emission of the sun's rays with the obvious symptoms corresponding with that circumstance; leaving it to others to apply the subject to such useful œconomical purposes as may be found to have any relation to them: at any rate, he cannot relinquish the hope that astronomy will ultimately supply us with the means of deriving certain prognostics of the temperature of the seasons from accurate observations on the quantity of the light we receive from the sun.

On an improved Reflecting Circle. By Joseph de Mendoza Rios, *Esq. F.R.S.* Read June 4, 1801. [*Phil. Trans.* 1801, *p.* 363.]

The great utility of Hadley's quadrant in practical astronomy, and particularly in navigation, has given rise to several improvements of that valuable instrument, of which some account is premised in the present paper. The first of these is due to the celebrated Tobias Meyer, who, by completing the limb of the sextant into a whole circle, and adding an horizon index, enabled us to repeat the observations, so as to ascertain the double, triple, and even a greater multiple of the angles; by which means the errors of division or eccentricity in the instrument can be reduced in the inverse ratio of the repetition of the observations, so as to arrive at any degree of approximation that may be required.

Some imperfection still remaining as to the manner of rendering the glasses parallel, so as to produce the exact coincidence of the images, the Chevalier de Borda contrived a method of rendering this exact parallelism of less consequence, by substituting the immediate

observation of the angular distance of the two objects to that of the coincidence of their images in one field. In his instrument the telescope is fixed at such a distance from the centre that the rays of light may arrive at the centre-glass both from the right and the left: double distances and cross observations are thus easily obtained, which essentially correct any imperfection in the construction of the instrument.

The peculiar advantages of these improvements being chiefly to afford the means of multiplying the observations of the distance required, Mr. Mendoza has directed his attention to some further improvements, which he thought might be deduced from the same principle. He accordingly favours us with an account of his new reflecting circle, of which a distinct idea can only be obtained by an inspection of the three accurate delineations which accompany his paper.

One of the additions is a compound handle, which facilitates the holding the instrument with the same ease in every direction. But the chief improvement appears to be a divided circle, moving round the centre, within, and close to the graduated limb, and capable of being alternately attached to each of the indexes. This the author calls the *Flying Nonius;* and shows how in every direction the two divisions may be made to exhibit the number of degrees on the limb, and of the minutes and seconds on the flying nonius. The manner in particular of making the crossed observations, by connecting the limb and the nonius alternately with the centre and horizon indexes, is here fully explained. And lastly, a small graduated semicircle is added to the horizon index, the use of which is to prepare the instrument previous to an observation, so as to facilitate the operation of bringing the images to coincide in the field of the telescope.

Observations and Experiments upon Dr. James's *Powder; with a Method of preparing, in the humid Way, a similar Substance. By* Richard Chenevix, *Esq. F.R.S. M.R.I.A.* Read June 4, 1801. [*Phil. Trans.* 1801, *p.* 375.]

From the experiments of Dr. Pearson on the nature of Dr. James's powder, published in the 81st volume of the Philosophical Transactions, our author infers that the mode in which it is prepared is far from being the best that the present improved state of chemical knowledge might afford; the use of fire in delicate processes, whether analytical or synthetical, being in general thought inferior to those performed in the humid way.

This powder, we are told, is prepared by mixing equal quantities of bone shavings (or phosphate of lime) and crude antimony, and calcining them together in an intense heat.

Here it is observed, that the portion of oxide of antimony, which is not volatilized in the process, becomes in a great measure insoluble in all acids. The humid process which Mr. Chenevix recommends as preferable to the above, consists in dissolving together or separately,

in the least possible portion of muriatic acid, equal parts of the white oxide of antimony and phosphate of lime; after which, pouring this solution gradually into distilled water previously alkalizated by a sufficient quantity of ammonia, a white and abundant precipitate will be produced, which, being well washed and dried, is the substitute he proposes for James's powder. A few observations are added on the theory of this combination; as also an assertion, that this powder, administered as a medicine, perfectly agreed in its general effects with James's powder, and the pulvis antimonialis, often prescribed in lieu of it, with this advantage, that being more mild, it may be given in larger doses, without producing the nausea or other stimulating symptoms that usually attend it.

Case of a young Gentleman, who recovered his Sight when seven Years of Age, after having been deprived of it by Cataracts, before he was a Year old; with Remarks. By Mr. James Ware, *Surgeon. Communicated by* Maxwell Garthshore, *M.D. F.R.S.* Read June 11, 1801. [*Phil. Trans.* 1801, *p.* 382.]

The subject of this case was the son of a clergyman in Somersetshire, who in his early infancy had every appearance of being a healthy, perfect child; but, when about a year old, was accidentally observed to be deprived of sight. A surgeon in the country pronounced that he had a complete cataract in each eye; and Mr. Ware, on being consulted, did not hesitate to decide that the only cure would be the removal of the opaque crystalline humour; but he added, that he did not think the child would be fit for the operation until he was at least thirteen or fourteen years of age. At the age of seven, however, the child's parents brought him to London, in order to enable Mr. Ware to form an opinion from his own observation. A recent case, in which this eminent operator had succeeded to restore sight to a youth about fourteen years of age, without extracting the cataract, but merely by making a large puncture in the capsule, so as to bring the opake crystalline into free contact with the aqueous and vitreous humours, having induced him to retract his opinion concerning the necessity of extracting the cataract, he proposed to perform the above operation immediately on one of the eyes of this new patient. This he effected without giving much pain; and in a few days the child described without hesitation all the objects that were set before him.

The author now draws a comparison between this case of restored sight and those described by Mr. Cheselden in the 35th volume of the Philosophical Transactions; and finding a considerable deviation in the results, he is induced to form several conclusions, which differ materially from those of his predecessors. These are briefly, That when children are born blind, in consequence of having cataracts in their eyes, they are never so totally deprived of sight as not to be able to distinguish colours:—that they have likewise some perception of distances; and that hence, when they recover their sight,

they can immediately form some judgement both of colours and distances, and even of the outline of strongly defined objects.

That when children have been born with cataracts, the crystalline humour has generally been found, either in a soft or a fluid state; and that in these cases, if the capsule be simply punctured with a couching-needle, there is reason to expect that the opaque matter will sooner or later be absorbed, and the sight be restored, and that should any opacity in the capsule itself render this operation ineffectual, the other, viz. that of extraction, may still be recurred to with every prospect of success.

Lastly, that this operation of couching being much more easy than that of extraction, it may be attempted at a very early period; and that thus the benefit of education may be afforded to children much sooner than if they were to wait till the proper age for extraction.

Mr. Ware acknowledges in a note, that about a month after the above operation he couched the other eye of his young patient, but that he did not prove equally successful: this he ascribes to some opacity in the capsule, which was incapable of being absorbed. The eye, however, he adds, remained as fit as ever for another operation.

An Account of some Galvanic Combinations, formed by the Arrangement of single metallic Plates and Fluids, analogous to the new Galvanic Apparatus of Mr. Volta. *By Mr.* Humphry Davy, *Lecturer on Chemistry in the Royal Institution. Communicated by* Benjamin Count of Rumford, *V.P.R.S.* Read June 18, 1801. [*Phil. Trans.* 1801, *p.* 397.]

Those who have attended to the latest experiments on galvanism, will recollect that the combinations hitherto used in that curious process consist of a pile of successive pairs of two metals, or of one metal and charcoal, and a stratum of fluid between each pair; and that the agencies of these combinations have been generally ascribed to the different powers of the metals to conduct electricity. Our author in the present paper states some arguments founded on experiments, from which it appears that an accumulation of galvanic influence, exactly similar to that produced in the above-mentioned pile, may be effected by the arrangement of single metallic plates, or arcs, between strata of different fluids. What first led to the discovery was the observation that the galvanic effects were readily produced when the metallic pairs were alternated with acids or other fluids capable of oxidating one only of the metals of the series. Double plates, for instance, composed of silver and gold, produced galvanic action when placed in contact in the common order with cloths moistened in diluted nitric acid; and plates of copper and silver when nitrate of mercury was used. It was hence inferred that galvanic effects might be produced if single metallic plates could be connected together by different fluids, in such a manner that one of their surfaces only should undergo oxidation, the arrangement in other respects being regularly progressive.

The first experiments were made with tin, small plates, or arcs, of which were made to alternate with acid and water. About twenty sets of these produce a galvanic battery, in which the wire from the oxidating surface of the plates evolved hydrogen, and that from the non-oxidating surface (when of silver) deposited oxide. The second series consisted of plates, or arcs, of silver, copper, or lead, placed alternately between cloths steeped in water, and in solution of sulphuret of potash. The effects of this combination were much more perceptible than those of the preceding. And a still more powerful battery was obtained by using metallic substances oxidable in acids, and capable of acting on solutions of sulphurets, and connecting them with oxidating fluids, and solutions of sulphurets of potash, in such a manner that the opposite sides of every plate may undergo different chemical changes. How this is to be effected is here explained at length, and an apparatus, contrived by Count Rumford, is lastly mentioned, for facilitating and giving permanency to the alternate succession of the different substances, so as to prevent, particularly in the fluids, the interference with each other, which would materially affect the results.]

A Continuation of the Experiments and Observations on the Light which is spontaneously emitted from various Bodies; with some Experiments and Observations on Solar Light, when imbibed by* Canton's *Phosphorus. By* Nathaniel Hulme, *M.D. F.R.S. and A.S.* Read June 18, 1801. [*Phil. Trans.* 1801, *p.* 403.]

A short description is here premised of an apparatus for exposing luminous bodies to different kinds of air, which, in addition to the well-known glass phial or tube inverted in water, consists in a small stand, to the top of which the luminous substance is fixed, and thus inserted into the inverted phial, into which the species of air to be employed is previously let up to the quantity of about eight ounces.

With these instruments a copious set of experiments has been made, of which the following are the principal results.

In common or atmospherical air, all the objects which abound with spontaneous light in a latent state, such as herrings, mackerel, &c., do not emit it when deprived of life, except from such parts as have been some time in contact with the air. Nor does the blast of a pair of bellows increase this species of light, as it does that which proceeds from combustion.

Oxygen gas does not act upon this kind of light so as to render it much more vivid than atmospherical air. And as to azotic gas, which is incapable of supporting light from combustion, it is remarkable that it should be so favourable to the spontaneous light emitted from certain fishes, as to preserve its existence and brilliancy when immersed in it, while it prevents the flesh of herrings and mackerel from becoming luminous, and extinguishes the light proceeding from rotten wood.

* See Phil. Trans. for 1800, p. 161.

Hydrogen gas in general prevents the emission of spontaneous light, and also extinguishes it when emitted; but at the same time it does not hinder its quick revival when the subject of the experiment is again exposed to the action of atmospherical air.

The carbonic acid gas has also an extinguishing property; but in general the light will soon return if the subject of the experiment be replaced in the open air. Hepatic gas extinguishes spontaneous light much sooner than carbonic acid gas, and the light is much longer in returning when the subject is exposed in atmospheric air. Nitrous gas prevents the emission of light even to such a degree that a long subsequent exposure to common air cannot restore it. A vacuum suspends the emission of spontaneous light, but it returns immediately on the re-admission of air.

A few experiments on solar light, when imbibed by Canton's phosphorus, are here subjoined, from which we learn in general that this light is subject to the same laws with respect to the influence of heat and cold, as the spontaneous light of fishes, rotten wood, and glow-worms, of which an ample account is contained in the author's paper on this subject, already published in the Transactions.

Experiments on the Chemical Production and Agency of Electricity. By William Hyde Wollaston, *M.D. F.R.S.* Read June 25, 1801. [*Phil. Trans.* 1801, *p.* 427.]

The power of Professor Volta's pile to produce galvanism is now supposed to depend on the disposition of one of the metals employed, to be oxidated by the fluid interposed; but a doubt is still entertained whether that oxidation depends on electricity set in motion by the contact of the metals, or whether the electricity be excited by some chemical action of the substances used. The experiments described in the present paper seem to favour the latter opinion, viz. that a chemical agency is the cause of the effects produced.

They go principally to prove that this chemical agency of common electricity is the same as the power excited in Professor Volta's apparatus, likewise by chemical means. The production of airs by the electric pile is here imitated, and even the appearance of two currents is produced, by occasioning the electricity to pass by fine points of communication on both sides of the water at the same time, each wire yielding both hydrogen and oxygen gas.

It is also observed, that in the same manner as in Professor Volta's apparatus, there is manifestly a disposition to oxidate on the positive side alone; and that although oxygen gas be given as well as hydrogen by the negative wire, that wire is never perceptibly oxidated; while the positive one, when of silver, uniformly shows a stream of oxide proceeding from it. This is accounted for by the supposition that the difference between these two effects is owing to the greater intensity of common electricity in the method hitherto employed; and it is ultimately inferred that galvanism probably differs solely in quantity and want of intensity from common electricity.

Further Observations on the Effects which take place from the Destruction of the Membrana Tympani of the Ear; with an Account of an Operation for the Removal of a particular Species of Deafness. By Mr. Astley Cooper. *Communicated by* Everard Home, *Esq. F.R.S.* Read June 25, 1801. [*Phil. Trans.* 1801, *p.* 435.]

The former part of this paper may be considered as a continuation of a preceding one by the same author, printed in the last volume of the Philosophical Transactions, and contains an enumeration of many more facts and circumstances, from which we gather that an aperture in the Membrana Tympani does not essentially diminish the power of the ear, and that even a complete destruction of that membrane is not followed by total deafness.

The causes by which it may be injured are here further inquired into, and are found chiefly to be a suppuration in the Meatus Auditorius, and any kind of external violence; such as blows on the side of the head, the forcible introduction of extraneous substances into the ear, &c.

Mr. Cooper proceeds next to describe the remedy he has, in consequence of his repeated observations that an aperture in the Membrana Tympani does not injure the ear, thought fit to apply to one particular species of deafness; namely, that which arises from an obstruction in the Eustachian tube. After enumerating the causes which most frequently produce these obstructions, such as colds, which often affect the parts contiguous to the orifice of this tube, ulcers in the throat, extravasation of blood, and uncommon strictures in the tube, the author proceeds to describe his operation, which consists simply in puncturing the membrane, with very little pain to the patient, and with instant relief to the disorder.

Several cases are described in which the operation has proved successful.

The criteria are next mentioned by which it may be known whether this tube be closed or open: and lastly, those kinds of deafness are enumerated in which the operation is not likely to produce any salutary effect. These are, when the auditory nerve is affected; when there is any alteration in the contents of the labyrinth; and when in general any derangement takes place which does not immediately affect the Eustachian tube.

The Croonian Lecture. On the Power of the Eye to adjust itself to different Distances, when deprived of the Crystalline Lens. By Everard Home, *Esq. F.R.S.* Read November 5, 1801. [*Phil. Trans.* 1802, *p.* 1.]

Its object is to state some facts and observations in support of an opinion advanced by the author in a former Lecture, that the adjustment of the eye to see objects at different distances does not depend upon any internal changes in the crystalline lens.

Before he proceeds, Mr. Home pays a due tribute of praise and

gratitude to our late ingenious brother, Mr. Ramsden, to whom he says he is chiefly indebted, not only for the information which was necessary to enable him to prosecute his investigations upon the subject of vision, but also the zeal which influenced his early exertions in the philosophical career.

The opinion here alluded to was brought forward in Mr. Home's Lecture for the year 1794, and was founded upon experiments which seemed to prove that the removal *of the crystalline lens does not deprive the eye of the power of seeing distinctly at different distances.*

An additional case is here mentioned of a man who had a cataract extracted from each of his eyes, and yet preserved a considerable range of vision.

In the Bakerian Lecture of last year, Dr. Young, having entered minutely into the inquiry, thought himself authorized to doubt the above inference; and in order to insure the accuracy of the experiments he intended to make on the subject, he constructed an optometer upon the principle of that of Dr. Porterfield, by which he could ascertain the different focal lengths, and hence the power of adjustment of every eye. The result of his experiments was, that eyes deprived of the crystalline lens have lost their power of adjustment.

This difference of results induced Mr. Home to reconsider the subject, and having sent for the man from whose eyes he had last extracted the cataracts, he repeated the experiments with Dr. Young's optometer, somewhat simplified by leaving out the lens which was placed before the eye. With this instrument that man was unquestionably found to have distinct vision at different distances, the nearest focus being at only 8·3 inches, and the furthest at 13·3 inches, while with Dr. Young's optometer he could never observe any difference whatever.

Besides this individual, others, whose eyes had never been disordered, tried the effects of both optometers; and it should seem, from the various impressions produced upon them, that the contradiction in the above results depends chiefly, if not entirely, on the difference of the instruments.

The Bakerian Lecture. On the Theory of Light and Colours. By Thomas Young, *M.D. F.R.S. Professor of Natural Philosophy in the Royal Institution.* Read November, 12, 1801. [*Phil. Trans.* 1802, *p.* 12.]

Although the mode, much practised by the ancients, of accounting for a variety of phænomena by a preconceived hypothesis, be, if not wholly exploded, at least greatly discountenanced by modern philosophers; yet it must be owned that when a number of facts have been collected and duly ascertained, it cannot but be conducive to the extension of knowledge, to arrange them under certain heads, and, if possible, to ascribe them to some general cause; and that with men who are candid and not over-tenacious, even an error in

such a proceeding may often be the means of eliciting some further information, which progressively may tend to the advancement of science. The immortal Newton has given us a striking example of this in his Theory of Light, which, should the principle he assumed prove ultimately erroneous, his investigation and mode of reasoning will yet remain an everlasting monument of acuteness and ingenuity, which it is likely will ever be found the best source of information to those who shall engage in this delicate branch of natural philosophy.

Under this impression, Dr. Young, having resolved to contemplate the subject of light and heat, in the present Lecture, proposes to take a general survey of what is extant, using the materials which, chiefly through Newton's means, are now at hand, and at the same time to add some new experiments of material importance in the investigation, in hopes thereby to establish a general principle which may apply to all the phænomena hitherto discovered.

The Newtonian system of emanation, though illustrated in so masterly a manner by its author, partly on account of the stupendous velocity it implies, has been ever thought liable to difficulties, which could not be satisfactorily obviated. Accordingly another hypothesis, namely, that of an ethereal fluid, producing its effects either by an undulatory motion or by a continued pressure, had been substituted by some, without however entering in a methodical manner into the abstruse disquisition necessary to establish their theories. This arduous disquisition our author engages in, in favour of the undulatory system; and it is no less curious than satisfactory, that, in carefully examining the writings of Newton, there are abundance of passages which prove that he was strongly impressed with ideas which singularly favour this theory.

In the first part of the Lecture, our author enumerates these passages, and adduces them in support of the three following hypotheses. 1. That a luminiferous ether, rare and elastic in a high degree, pervades the whole universe. 2. That undulations are excited in this ether whenever a body becomes luminous. And, 3. That the sensation of different colours depends on the frequency of vibrations excited by light in the retina. It is here to be observed that, speaking of the motion of this ether, Newton uses the term vibration instead of undulation, which two words manifestly convey different meanings, the one being the alternate motion of a pendulum, and the other that of waves which protrude each other. It is likewise obvious, as to the motion of the retina, that it must rather be of the vibratory than of the undulatory nature, the frequency of the vibrations depending on the constitution of the substance limited to the sensation of colours.

These three hypotheses, which may be called essential, are here shown to be literally parts of the more complicated Newtonian system. But a fourth is now advanced, which appears diametrically opposite to that of Newton, and differs in some measure from any that has been hitherto proposed by other writers, although the author does not consider this difference as affecting in any degree its ad-

missibility. It assumes that all material bodies have an attraction for the ethereal medium, by means of which it is accumulated within their substance, and for a small distance around them, in a state of greater density, but not of greater elasticity.

The whole theory is now applied to the phænomena, in Nine Propositions, together with several scholia and corollaries. As the arguments on which the doctrine rests cannot be abbreviated without impairing their evidence, we must content ourselves with merely enumerating the heads, adding, however, a few observations which may facilitate the understanding of the main object of the inquiry.

All impulses, says the author in the First Proposition, are propagated in a homogeneous elastic medium, with an equable velocity. The truth of this theorem has been mathematically demonstrated by various writers, the actual motion being considered as very minute. Prop. 2. An undulation conceived to originate from the vibration of a single particle must expand through a homogeneous medium in a spherical form, but with different quantities of motion in different parts. Prop. 3. A portion of a spherical undulation, admitted through an aperture into a quiescent medium, will proceed to be further propagated rectilineally in concentric superficies, terminated laterally by weak and irregular portions of newly-diverging undulations. The chief purport of this last Proposition is to obviate the objection, that if light were the effect of a widely-expanded fluid put in motion by an impulse, it would, like the waves of water, and air in the instance of sound, spread laterally from the original direction of the motion, and agitate the contiguous quiescent medium; by which means we ought to see objects as well as we hear sounds, behind an opaque body. It is here alleged that, according to Newton's own words, sounds diverge much less than the waves of water, the air being more rare and elastic; and that in the very rare luminous medium, after its undulations have passed by an opaque substance, they will indeed diverge a little from their first direction, but this in so small a degree as to be almost imperceptible; whereas the loss of even this small degree of impulse will make the progressive undulatory beam appear somewhat contracted. It is no small confirmation of the theory, that this effect perfectly agrees with the result of an experiment mentioned by Sir Isaac Newton; though, having adopted a different principle, he used it rather as an objection to the undulatory system. The subject of this Proposition having always been considered as the most difficult part of the last-mentioned system, the author has taken particular pains to clear it as much as possible from all objections.

The mere recital of the enunciations of the four next Propositions will probably enable those at all conversant with the science of optics, to perceive in what manner the author means to explain, according to his theory, the important phænomena of Refraction, Reflection, and Colours. They are stated in the following manner.—When an undulation arrives at a surface which is the limit of media of different densities, a partial reflection takes place, proportionate in force to

the difference of the densities. This, it is thought, may be well illustrated, if not demonstrated, by the analogy of elastic bodies of different sizes. When an undulation is transmitted through a surface terminating different media, it proceeds in such a direction that the sines of the angles of incidence and refraction are in the constant ratio of the velocity of propagation in the two media. When an undulation falls on the surface of a rarer medium so obliquely that it cannot be regularly refracted, it is totally reflected at an angle equal to that of its incidence. And if equidistant undulations be supposed to pass through a medium of which the parts are susceptible of permanent vibrations somewhat slower than the undulations, their velocity will be somewhat lessened by this vibratory tendency; and the more so in the same medium, the more frequent the undulations. If we ascribe the sensation of colours to the different velocities of the coloured beams or undulations, this last Proposition will afford a solution to the phænomena of dispersion according to the new system.

When two undulations from different origins coincide either perfectly or very nearly, in direction, their joint effect is a combination of the motions belonging to each. This is the Eighth Proposition, which, at first sight, appears so consistent with the most obvious mechanical principles, as scarcely to need any illustration; yet its extensive utility in explaining the phænomena of colours renders it perhaps the most important in the lecture. In a first corollary the author treats of the colours of striated surfaces, where, after showing in what manner these depend on the breadth of the undulations in proportion to the distance and position of minute surfaces, it is shown from original experiments in what manner this circumstance affords a very strong confirmation of the theory. But a still more interesting coincidence is shown in the second and third corollaries, which treat of the colours of thin plates, and of thick plates. It is here explained by what means the breadth and duration of the respective undulations may be deduced from Newton's measures of the thicknesses reflecting different colours; and the law of variation of colour, in consequence of the change of obliquity, which is very embarrassing on every other supposition, and had never been reduced to any analogy, is referred to a simple and necessary consequence of the author's theory.

The whole visible spectrum being estimated to be comprised within the ratio of 3 to 5, the undulations of red, yellow and blue appear to be related to each other in magnitude as the numbers 8, 7, and 6. On these data a table is constructed, showing for each primitive colour, and the intermediate ones between each pair of them; 1. The length of an undulation in parts of an inch in air. 2. The number of undulations in an inch. And 3. The number of undulations in a second. All these numbers agreeing accurately with the phænomena, will probably be considered as a strong evidence in favour of the theory. The appearances of colours in inflected light are likewise explained in a subsequent corollary.

The last Proposition may be considered as the general result of the whole investigation; in consequence of which, Dr. Young thinks him-

self authorized to assert, without hesitation, that radiant light consists in undulations of the luminiferous æther. The general inferences he draws from his arguments are, that it is clearly granted by Newton that there are undulations, although he denies that they constitute light; and that it being shown in the three first corollaries of the Eighth Proposition, that all cases of the increase or diminution of light are clearly referable to an increase or diminution of such undulations, and that all the affections to which the undulations would be liable are distinctly visible in the phænomena of light, it may therefore be very logically inferred that the undulations are light.

Dr. Young proceeds to attempt the removal of some apparent difficulties in the system which he has adopted; and concludes with a summary comparison of light with heat, which he supposes to differ from it only in the magnitude and frequency of its undulations or vibrations.

An Analysis of a mineral Substance from North America, containing a Metal hitherto unknown. By Charles Hatchett, *Esq. F.R.S.* Read November 26, 1801. [*Phil. Trans.* 1802, *p.* 49.]

This substance, which was lately found among the minerals in the British Museum, appears by an entry in Sir Hans Sloane's Catalogue, to have been sent to him with various specimens of iron ores, by Mr. Winthrop of Massachusetts, whence it is conjectured that it is the produce of that province. Its resemblance to the Siberian chromate of iron first attracted Mr. Hatchett's notice. It is of a dark brownish gray; its longitudinal fracture is imperfectly lamellated, and the cross fracture shows a fine grain. Its lustre is vitreous; it is moderately hard, and very brittle.

The analysis was conducted with all the chemical agents usually applied upon those occasions; and the whole process is minutely described in the paper. From these experiments we learn that this ore consists of about one quarter of iron, and three quarters of a substance hitherto unknown, but now proved to be of a metallic nature, both by the coloured precipitate which it forms with prussiate of potash and with tincture of galls, and by the colour which it communicates to phosphate of ammonia, or rather to concrete phosphoric acid when melted with it.

From the experiments made with the blowpipe, it seems to be one of those metallic substances which retain oxygen with great obstinacy, and are therefore of difficult solution. That it is an acidifiable metal appears from the circumstance of the oxide turning litmus paper red, expelling carbonic acid, and forming combinations with the fixed alkalies; but in many points which are enumerated, it is manifestly very different from the acidifiable metals hitherto known, such as arsenic, tungsten, molybdena, and chromium, and it appears to differ still more from the lately discovered metals known by the names of uranium, titanium, and tellurium.

The iron contained in this ore is in the same state as it is found in Wolfram, namely, brown oxide; and this oxide is mineralized by the new metallic acid in the same manner as the oxides of iron and manganese are mineralized by the tungstic acid, or rather oxide. Several facts which have appeared in the course of this investigation seem to prove that this new metal differs from tungsten and the other acidifiable metals by a more limited extent of oxidation; for, unlike these, it seems to be incapable of retaining oxygen sufficient to enable the total quantity to combine with fixed alkalies.

All that can be said at present as to the uses of this metal is, that an olive-green prussiate, and an orange-coloured gallate they yield, are both very fine colours, which, as they do not appear to fade when exposed to light and air, may probably be employed with advantage as pigments. The author lastly hazards a conjecture, that several of the newly discovered metals and other substances, which are now considered as simple, primitive, and distinct bodies, will, upon further examination, turn out to be compounds. Meanwhile as the new metal here described appears hitherto distinct from all the others, it cannot but be expedient to distinguish it by a proper appellation; and the least objectionable that has hitherto occurred, is that of Columbium.

A Description of the Anatomy of the Ornithorhynchus paradoxus. *By* Everard Home, *Esq. F.R.S.* Read December 17, 1801. [*Phil. Trans.* 1802, *p.* 67.]

Two specimens of this curious animal, lately brought from New South Wales, the one male and the other female, and both full grown and perfect, having been submitted to the inspection and close examination of Mr. Home, by Sir Joseph Banks, this gentleman has availed himself of the favourable opportunity to draw up the full account of all that is hitherto known concerning its habits, of its external appearance, and internal structure now before us.

The animal has hitherto been only found in the fresh-water lakes in the interior parts of the above-mentioned country. It does not swim upon the surface of the water, but comes up occasionally to breathe. It chiefly inhabits the banks of these lakes, and is supposed to feed in the muddy places which surround them; but the particular kind of food on which it subsists is not known.

As in its anatomical structure this animal differs in many respects from other quadrupeds, those who interest themselves in inquiries of this nature will be gratified to find in this paper a comparative view of those deviations; and when they have satisfied themselves in this respect, they will probably allow that it is long since facts so singular and novel have been brought to light respecting the science of comparative anatomy. Being obliged, as is usual in all descriptive communications of this kind, to refer to the paper itself for an adequate estimate of ts merits, we shall dwell briefly upon a few of

the most striking peculiarities, from which some idea may be gathered of the importance of this performance.

Besides four grinding teeth, one at each end of the two jaws, the animal has two small pointed horny teeth upon the projecting part of the posterior portion of the tongue, the points of which are directed forwards. These, it is thought, are intended to prevent the food from being pushed into the fauces during the process of mastication; and such have as yet been observed in no animal except the flamingo, which has a row of similar small teeth at each side of the tongue.

The fifth pair of nerves, which supplies the muscles of the face, and extend to the membrane that covers the bills, were found uncommonly large; whence it is inferred that probably the sensibility of the different parts of this bill is very great, and that being capable of nice discrimination in its feeling, it answers in some respects the purposes of a hand.

A striking peculiarity is observed in the structure of the bones of the chest. The scapulæ, which are of an uncommon shape, are not connected with the chest, but with a bone placed above the sternum, the upper part of which answers the purpose of clavicles. The cartilages also of the ribs are not placed next the sternum but between two portions, and about the middle of each rib: and the false ribs have their cartilages terminated by thin bony scales, which slide on one another in the motions of the chest. From this singular construction, it appears that the capacity of the chest can undergo a very considerable degree of contraction and dilatation.

On each of the hind legs of the male, at the setting on of the heel, is a crooked, strong, sharp-pointed spur, which is retractile, but may be considerably extended. Its use is conjectured to be the confining the female in the act of copulation: but in nothing, perhaps, does this animal differ more from the other quadrupeds than in the parts of generation. Externally there is no appearance of these organs in either sex, the orifice of the anus being a common opening to the rectum and prepuce in the male, and to the rectum and vagina in the female. The testicles are situated in the cavity of the abdomen, the glans penis is double, one part being directed to the right and the other to the left. The female has no regularly formed uterus, but towards the end of the vagina are two openings, each leading into a cavity resembling the horn of the uterus in quadrupeds, but terminating in a fallopian tube, which opens into the capsule of an ovarium. From various circumstances attending this singular configuration, and from some analogy it bears to the similar organ in birds, our author is inclined to believe that this animal will be found to be oviparous in its mode of generation.

On the Independence of the analytical and geometrical Methods of Investigation; and on the Advantages to be derived from their Separation. By Robert Woodhouse, *A.M. Fellow of Caius College, Cambridge. Communicated by* Joseph Planta, *Esq. Sec. R.S.* Read January 14, 1802. [*Phil. Trans.* 1802, *p.* 85.]

The author, in the prefatory part of this paper, points out the difference between the two methods of solving problems,—the one using lines and diagrams as the signs of quantity, and making an individual to represent a genus; and the other employing generic terms and signs, which bear no resemblance to the things signified: and insists that, in order to make the process of deduction distinct, exact, and luminous, only one of the two methods ought to be adhered to. This, he says, has not been sufficiently attended to, expressions and formulas of the two methods having often been blended together, the consequence of which has been much ambiguity and paradox; since the true method of combining algebraical formulas cannot be well understood, unless we duly attend to their true analytical source and combination. To show that the language of algebra need not be infected with the mode of expression adopted by geometricians, and that it is of itself an adequate instrument of argumentation, is the principal object of Mr. Woodhouse's paper. And he declares that he has entered on this inquiry, not merely for the sake of gratifying speculative curiosity, being firmly of opinion that the process of calculation will be much more direct, sure, and expeditious, if it be duly freed from all foreign encumbrances.

In order to illustrate and confirm this opinion, he has selected a few cases from those expressions and formulas which are supposed to require for their solution the aid of geometrical theorems, and of the properties of curves.

From purely analytical principles he has given demonstrations; 1st, of the integrals of a series for the sine of an arc in terms of the arc; 2ndly, of the expression for the root of a cubic equation in the irreducible case; 3rdly, of the resolution of the series $x^n \mp a^n$, &c., into quadratic factors; and, 4thly, of the series for the chord, sine, cosine, &c. of a multiple arc, in terms of the chord, sine, &c. of the simple arc. These demonstrations the author presumes to be direct and rigorous, which advantages, he asserts, are in a great measure owing to the deductions being expressed in algebraical language, and effected throughout by analytical processes.

The paper concludes with a brief comparison of the ancient geometry and modern analysis respecting the advantages of perspicuity and commodious calculation. The result of this comparison is, that some of the excellencies of the former science have been exaggerated, and others deemed essential, which in fact are only accidental. If the object of mathematical study be chiefly recreation, and the exercise of our mental faculties, our author admits that the finest examples of reasoning are to be found in the works of the ancient

geometricians; but he further insists that, for the investigation of abstruse and latent truth, and the evolution of intricate problems, the analytical method is on every consideration to be preferred to the geometrical.

Observations and Experiments upon oxygenized and hyperoxygenized Muriatic Acid; and upon some Combinations of the Muriatic Acid in its three States. By Richard Chenevix, *Esq. F.R.S. and M.R.I.A.* Read January 28, 1802. [*Phil. Trans.* 1802, *p.* 126.]

The author introduces the subject of his paper by stating that Mr. Berthollet, having observed a large portion of common muriate of potash to be always produced along with the hyperoxygenized muriate, had formed an ingenious conjecture, that the quantity of oxygen, relatively to the acid, was greater in the salt than in disengaged oxygenized muriatic acid; but that no experiments having appeared since the year 1788 to prove this assertion, he was induced to examine the properties of the salt, and the nature of the acid it contains. He next mentions such authors as have treated any part of his subject; and intimates that Mr. Hoyle of Manchester appears to him to be the chemist, who, after Mr. Berthollet, has approached nearest to the truth. He then proceeds to describe the means by which he has determined that the acid contained in his hyperoxygenized muriate of potash is, in fact, an acid *sui generis*; and those by which he arrived at the proportion of oxygen. After which he treats of the saline combinations of oxygenized and hyperoxygenized muriatic acids.

To determine the proportion of oxygen in hyperoxygenized muriatic acid, he distilled one hundred grains of hyperoxygenized muriate of potash in a coated glass retort, and collected one hundred and twelve cubic inches of oxygen gas, = 38·3 grains. He then precipitated by nitrate of silver the salt which remained in the retort, and a small portion of it that had been volatilized into the tube, and obtained a quantity of muriate of silver, corresponding with twenty of muriatic acid; and hence he concluded that one hundred parts of hyperoxygenized muriatic acid contained,

Oxygen	65
Muriatic acid	35
	100

He then passed a current of oxygenized muriatic acid through a solution of potash, and distilled the liquor to dryness in an apparatus, by which he could ascertain whether there was any disengagement or absorption of oxygen from the liquor or from the salt it held in solution. No oxygen was disengaged or absorbed; and hence it appears that the same quantity was now condensed in the hyperoxygenized muriate of potash as was originally contained in a relative quantity of oxygenized muriatic acid. The salt thus obtained, Mr. Chenevix, for the sake of brevity, calls *entire salt*. He analysed it,

and found it to contain common muriate of potash 84, hyperoxygenized muriate 16. But by the proportions established above, 16 hyperoxygenized muriate contain 6 of oxygen, and this, with the acid contained in the whole 100 of entire salt, gives the proportions,

Oxygen	16
Muriatic acid	84
	100

These proportions differ a little from those obtained by Mr. Berthollet and by Mr. Cruikshank; the former mentions 11 per cent. of oxygen, the latter 43. But Mr. Berthollet, in all probability, used an acid which already contained a little simple muriatic acid, or else he did not expel all the oxygen from his oxygenized muriatic acid by the light of the sun. And Mr. Cruikshank having made use of hyperoxygenized muriate of potash and muriatic acid, to obtain that which he examined, the result was a mixture of oxygenized and hyperoxygenized muriatic acid gases.

Having stated the proportions of the acids, the author passes on to the examination of the salts. Oxygenized muriates are decomposed at the very moment of their formation, and are resolved into common muriates and hyperoxygenized muriates. To prove this, Mr. Chenevix asserts that he always obtained the same proportion of muriate of silver, by pouring some nitrate of that metal into the recent liquor of the entire salt,* as into some that he had evaporated. But he concludes that the acid does really come into contact with the alkali, and unite with it, in the state of oxygenized muriatic acid, because ammonia is decomposed by a current of that acid; and ammonia (as is afterwards proved) is not decomposed either by common or by hyperoxygenized muriatic acid. From this experiment he concludes also, that hyperoxygenized muriatic acid has a much greater affinity than oxygenized muriatic acid to the salifiable bases.

Mr. Chenevix then passes to the examination of the hyperoxygenized muriates. These are all formed by the resolution of the elements of oxygenized muriates into common muriates and hyperoxygenized muriates. They have properties that characterize them fully. The acid is expelled by all acids, except the benzoic, acetic, acetous, boracic, prussic, and carbonic; and the order of affinity of the salifiable alkaline and earthy bases is potash, soda, barytes, strontia, lime, ammonia, magnesia, alumina, and silica.

The first species is, therefore, hyperoxygenized muriate of potash, which the author thinks can exist in two states. It was from this salt chiefly that he attempted to disengage the acid. If sulphuric acid be poured upon it, a crackling noise is heard, and an orange-coloured liquor, with greenish yellow fumes, is disengaged; but the acid cannot thus be obtained pure, as the heat necessary to bring it over is sufficient to decompose it. In attempting to distil this mixture a violent explosion ensued as soon as heat was applied. As a caution to those who would repeat the experiment, Mr. Chenevix describes an accident which happened to Dr. Vandier, by which that gentle-

man was dreadfully wounded, and was near losing his sight: by dropping the salt into sulphuric acid there is less danger of explosion at the beginning; but still the acid does not come over without decomposition. By cooling the first receiver with ice, the author thinks that he has obtained the acid in the form of little orange-coloured octahedral crystals.

Nitric acid produces nearly the same phænomena.

Muriatic acid decomposes the salt, and takes a part of the oxygen from the hyperoxygenized muriatic acid, and becomes oxygenized.

Phosphoric, tartareous, oxalic, arsenic, and citric acids decompose this salt with the help of heat.

Some attempts were made to combine diamond with oxygen, in the humid way, by means of this salt and this acid; but they did not prove successful. Caloric is mentioned as a considerable ingredient in this as in all hyperoxygenized muriates.

The proportions of the salt are,

Hyperoxygenized muriatic acid	58·3
Potash	39·2
Water	2·5
	100·0

The second species is hyperoxygenized muriate of soda. This salt Mr. Chenevix obtained pure by crystallizing in alcohol. It is decomposed by the same agents as the former species. It is deliquescent. Its proportions are,

Hyperoxygenized muriatic acid	66·2
Soda	29·6
Water	4·2
	100·0

A distinguishing character of the earthy hyperoxygenized muriates is their resemblance to their respective muriates, in point of solubility. The author at first despaired of being able to separate them from the muriates which accompany their formation; but phosphate of silver afforded him the means. Phosphate of silver decomposes all simple muriates, and the hyperoxygenized muriates remain alone in solution. It was thus he obtained them pure enough for analysis. He found the following proportions in each salt:

Third species. Hyperoxygenized muriate of barytes.

Hyperoxygenized muriatic acid	47·0
Barytes	42·2
Water	10·8
	100·0

Fourth species. Hyperoxygenized muriate of strontia.

Hyperoxygenized muriatic acid..........	46
Strontia	26
Water................................	28
	100

Fifth species. Hyperoxygenized muriate of lime.

Hyperoxygenized muriatic acid........	55·2
Lime............................	28·3
Water...........................	16·5
	100·0

The sixth species of hyperoxygenized muriate of ammonia cannot be formed by direct combination. By pouring a solution of carbonate of ammonia into a solution of any of the earthy hyperoxygenized muriates, the earth is precipitated with the carbonic acid, and hyperoxygenized muriate of ammonia remains in the liquor. This salt is decomposed at a low temperature, and has all the characters of the genus to which it belongs. It is a very striking example of the force of co-operating affinities which can unite this acid with ammonia; while oxygenized muriatic acid decomposes that alkali. It is also a proof of the different attractions exercised by these two acids toward the salifiable bases.

Hyperoxygenized muriate of magnesia is the seventh species. Its proportions are,

Hyperoxygenized muriatic acid........	60·0
Magnesia........................	25·7
Water...........................	14·3
	100·0

The author has not determined the proportions of the eighth species, viz. hyperoxygenized muriate of alumina, because this salt was always decomposed by phosphate of silver; and he imagines that hyperoxygenized muriate of silica does not exist.

He then states some corrections which he has made in the proportions of common muriates, respecting the quantity of water they contain. This he esteems to have been rated too high by the chemists who have examined them. He exposed a given weight of muriate of potash to a red heat, and examined it to know if any of the acid had been expelled. Some portion had been volatilized, and upon this correction he established the proportions he has announced in this paper.

He then passes to the metallic salts of this genus, a number of which he has formed by passing a current of oxygenized muriatic acid through water, in which the oxide was suspended. Copper, iron, lead, and silver, he readily combined with the acid. The last of these salts he considers with greater attention for two reasons: first, because it affords a very striking difference between the hyperoxygenized and common muriatic acids; and, secondly, on account of its extraordinary properties. This salt is soluble in three parts of water, crystallizes by cooling, is not affected by light, but is decomposed by all the weak acids, even vinegar. Rubbed in a mortar with about three-fourths of sulphur, it detonates by a very gentle pressure, and with so much violence that Mr. Chenevix estimates the expansive force at nearly ten times that of a mixture of sulphur and hyperoxygenized muriate of potash.

He then states two extraordinary and unforeseen productions of this acid: one was during an analysis of manachanite from Botany Bay, in which oxygen had passed from oxide of titanium into a combination of potash with muriatic acid, and formed hyperoxygenized muriate of potash; and the other was in distilling nitro-muriatic acid upon platina. He tried the action of manganese in the same manner as the titanium, but could not succeed; nor did nitric acid convert oxygenized into hyperoxygenized muriatic acid.

Mr. Chenevix states that Mr. Berthollet had proposed to consider muriatic acid as the radical of the other two, and says that oxygenized muriatic acid corresponds with nitrous and sulphureous acids, and hyperoxygenized muriatic acid with nitric and sulphuric.

Our author states the arguments in favour of the old and the new mode of denomination; and upon the consideration that many bodies called acids have not been proved to contain oxygen, and that of some the contrary has been demonstrated, he seems inclined to think that an impartial hearing of both sides of the question must, in the present state of chemical knowledge, decide in favour of

Muriatic radical, or some word of the same import,	instead of	Muriatic acid;
Muri*atous* acid,		Oxygenized muriatic acid;
Muri*atic* acid,		Hyperoxygenized muriatic acid.

Experiments and Observations on certain Stony and Metalline Substances, which at different Times are said to have fallen on the Earth; also on various Kinds of native Iron. By **Edward Howard, *Esq. F.R.S.*** Read February 25, 1802. [*Phil. Trans.* 1802, *p.* 168.]

In considering the copious contents of this paper, we shall find it convenient to distribute them under the four following heads: 1. The historical part; 2. The descriptive part; 3. The analytical part; and 4. General observations on the subject, and a comparison of these stones with other substances which seem to bear some affinity to them.

And, First, as to the historical part. Waving all former accounts, both of the ancients and moderns, of stones which, under the names of Ceraunia, Bœtilia, Ombria, Brontia, Belemnitæ, &c. were supposed to have fallen from heaven, of which accounts most are disproved, and others are involved in inexplicable obscurity, we may lay some stress on the instances adduced by Mr. King, in his late tract "concerning stones which are said to have fallen from the clouds;" and also on the evidence of the Abbé Bachelay, who laid before the French Royal Academy a stone, which he asserted had been found on the 13th of September, 1768, still hot, by persons who saw it fall; and that of Professor Barthold, who analysed and described a stone found near Ensisheim in Upper Alsace, under the unqualified name of Pierre de Tonnere. These observations and experiments of the Abbé and

the Professor are here quoted at some length; as our author proposes to draw a comparison between their results, and those he deduces from his own investigation.

The instances next in succession, and which are the principal objects of this paper, are: 1. The several stones, about twelve in number, which in the year 1794 were seen by several persons falling from the clouds near Sienna, in the midst of a violent thunder-storm, and eighteen hours after an enormous eruption of Mount Vesuvius. As an ample account of this extraordinary phænomenon is printed in the Philosophical Transactions for the year 1795, it will be needless to dwell here any longer upon it, than merely to observe that these stones are of a quality not found in any part of the Siennese territory; and that as to their being of volcanic origin, it is scarce credible that they could have been carried in the air to a distance of at least two hundred and fifty miles. 2. The second instance is that of the stone, weighing no less than fifty-six pounds, which, according to the attestation of several persons who profess themselves eye-witnesses, fell on the 13th of December, 1795, near Wold Cottage in Yorkshire, and was, when extracted from the depth of about eighteen inches in the earth, still warm, and smoking. Here too no similar stone is to be met with in any part of that county. The weather was mild and hazy, but there was no thunder or lightning the whole day. 3. The third instance, which comes perhaps better authenticated than the two preceding ones, is that of a number of stones which, after the explosion of a meteor on the 19th of December, 1798, fell about eighteen miles from Benares, in the East Indies. The account of this phænomenon is given in a very circumstantial manner by John Lloyd Williams, Esq. F.R.S. At the time when the meteor appeared, the sky was perfectly serene, nor had the smallest vestige of a cloud been seen for several days before and after the phænomenon. The largest of the stones, of which Mr. Williams had seen eight, weighed 2lb. 12oz. There are no volcanoes on the Continent of India, nor have any stones yet been met with in that part of the world which bear the smallest resemblance to those here described. Lastly, the fourth instance occurs in the collection of Baron Born, now in the possession of the Right Hon. Charles Greville*. In Born's catalogue the specimen here mentioned is described as a mass of iron found near Tabor in Bohemia; and in a note it is observed that credulous people assert it to have fallen from heaven on the 3rd of July, 1753: in fact, on comparing it with the Sienna and Yorkshire stones, there appeared sufficient reason to excite a suspicion of its being of the same nature.

We here anticipate the account given in the latter part of the paper of the two enormous masses of a substance which has been considered as native iron, the one weighing about 15 tons, observed in South America, and described by Don Rubin de Celis, whose account is inserted in the Philosophical Transactions for the year

* Since purchased by Government, and deposited in the British Museum.

1788; and the other discovered in Siberia, and described by Pallas. This latter the Tartars consider as a sacred relic which had descended from heaven.

II. For the descriptive part of the four stones which have been analysed by Mr. Howard, we are indebted to the Count de Bournon, who observes in general, that none of them are, or appear ever to have been, of any regular shape; and that when entire they are all coated with a black crust, the thickness of which however is very inconsiderable. The Benares stone being that which has the most striking mineralogical characters, obtained the preference in these descriptions, and served as an object of comparison in the account to be given of the others. The crust already mentioned, which is common to all, is of a deep black colour, and of an uneven surface. It strikes fire with steel, and frequently contains particles of native iron. The stone itself when broken is of a grayish ash-colour, and of a granulated texture: it appears evidently to be composed of four different substances; one of them, which is in great abundance, shows itself in the form of small spherical bodies of various sizes, of a gray colour, sometimes inclining to brown, perfectly opake, and so hard as to give faint sparks when struck with steel. Another of these substances is a martial pyrites, of a reddish yellow tinge, somewhat inclining to the colour of nickel. When powdered it is of a black colour, and not attractable by the loadstone. The third substance consists of small particles of iron, in a perfect metallic state. These, although they compose only about $\frac{1}{50}$th part of the whole stone, give, however, to the whole mass the property of being attractable by the magnet.

These three substances are united together by means of a fourth as a cement, which is nearly of an earthy consistence, and of a whitish gray. The specific gravity of the aggregate stone is 3352.

The constituent parts of the stone from Yorkshire are exactly the same as those of the above, except that its grain is finer, that the globules are more irregular in their shape, that the martial pyrites is in less, and the iron in greater proportion, and that the earthy cement is more compact. Its specific gravity = 3508.

The Sienna stone was more similar to that from Benares than the last mentioned; the particles of iron were in a somewhat greater proportion. It contained some particles of black oxide of iron; and likewise one single globule of a vitreous substance, of a pale yellow colour inclining to green, and of a hardness rather inferior to that of calcareous spar. The specific weight of the aggregate was = 3418.

Lastly, the stone from Bohemia was most similar to that from Yorkshire, except that it appeared to be totally free from any particles of pyrites, and on the other hand to have a much larger proportion of globules of native iron; many of which, perhaps, on account of the stone having remained longer in the earth, had undergone a degree of oxidation on their surfaces. Its specific gravity was = 4281.

From these descriptions we learn that these stones, though they have not the smallest analogy with any of the mineral substances already known, have a very peculiar and striking resemblance to each other; a circumstance surely which must excite the attention and

stimulate the endeavours of philosophers, and particularly of chemical analysts.

III. The foremost among these was Mr. Howard, who, in the third part of his paper describes the several methods by which he examined each of the constituent parts of these stones separately, avoiding thereby the mistakes of the Abbé Bachelay and Professor Barthold, who by making their experiments only upon the stones in the aggregate, obtained of course none but fallacious results.

The Benares stone being, as Count de Bournon had already mentioned, the most characteristic, was the first he undertook to examine. In analysing the crustaceous matter, in which some nickel was soon found to be contained, the process led to an investigation of the triple salt, described by Mr. Hermstadt as an ammoniacal nitrate of nickel, which was soon found to be a proper menstruum for discovering the presence of the last-mentioned metal. The presence of iron and nickel was manifestly discovered in their substance; but the quantity that could be obtained was so small that it was found impracticable to give the proportions of their constituent parts.

The pyritical part was next examined. The result of the analysis, the particulars of which cannot be abridged, was, that 16 grains contains iron 10½ grains, sulphur 2 grains, nickel nearly 1 grain, and extraneous earthy matter 2 grains: half a grain appears to have been lost in the process, owing probably to the impossibility of reducing the sulphur to the same degree of dryness as it existed in combination with the iron. The weight of the nickel, too, is a mere estimation, our acquaintance with that metal being as yet too imperfect to speak of it with accuracy, except as to its presence.

The third substance Mr. Howard examined was the native iron disseminated in the mass in small globules. Having reason to suspect that some nickel was likewise contained in this substance, he contrived an expedient for estimating its proportion, of which the following is a slight indication:—Finding that 100 grains of pure iron would yield about 145 grains of oxide, it would be a certain proof that the metal contains something which is either volatilized or left in the solution, if, under the same circumstances, it do not acquire the same proportionate weight. Hence when a metallic alloy of nickel and iron in known proportions is digested in nitric acid, it is plain that the deficiency of weight in the precipitated oxide of iron will be proportionate to the quantity of nickel contained in the alloy. By this means 25 grains of these metallic globules, being freed from earthy and other extraneous matter, left 23 grains of alloy, which were found to consist of 14 grains of pure iron, and 9 of nickel.

The small spherical bodies, equally dispersed throughout the mass, were the fourth objects of inquiry. The result of this analysis was, that 100 grains of the substance was decomposed into 50 silica, 15 magnesia, 34 oxide of iron, and 2½ oxide of nickel. By summing up these it will be found, that instead of the loss usual on these occasions, there was an excess of weight of 1½ grain; this is ascribed to the oxidizement of the iron.

Lastly, the earthy matter, forming a cement or matrix for the sub-

stances already examined, became the subject of investigation; and the mean result of two analyses gave, in 100 grains of the earth, 48 silica, 18 magnesia, 34 oxide of iron, and 2½ oxide of nickel.

When we observe that the three other stones were found to contain the same elements as that just now described, only in somewhat different proportions, we may be excused from detaining the Society with the particulars of their several analyses. Nor shall we here dwell upon Mr. Howard's manner of reconciling his results with those of the Abbé Bachelay and Prof. Barthold, any further than to state, that the inferences drawn are favourable to the supposition, that the stones they examined were of the same nature as those here described.

IV. In this section we collect some of Mr. Howard's principal observations on this curious subject. It must be admitted, that notwithstanding the concurring evidence we have of the fall of some of these substances from the atmosphere, yet the fact itself is so repugnant to what we know as yet of the operations of nature, that we are likely to pause a while before we shall venture to form any decided opinion on the subject. Had the fall of all these stones been attended with meteors, we should naturally combine the two phænomena; and in this case Mr. Howard contends, that as these meteors generally move in a direction nearly horizontal, and probably not very high in the atmosphere, the objection of the stones not striking deeper into the earth, owing to their accelerated velocity, would be obviated. The imperfect knowledge we have of the origin and nature of meteors may likewise be considered as an encouragement for inquiring further into this hypothesis.

Should these masses, after all, turn out to be the effects of some regular and simple operation of nature, it is likely that many more will be found on the surface of the earth, which will become the objects of future inquiry. Meanwhile, Mr. Howard has thought proper to take a comparative view, not only of the masses of what has been denominated Native Iron in South America and Siberia, but also of every other specimen that could be met with in our collections of the substances that came under the same denomination. For a description of these substances, together with various observations thereupon, we are likewise indebted to Count de Bournon, who seems to think, that were the particles of iron and nickel in the stones here examined so numerous as to be in contact with each other, and were the earthy particles interposed between them wholly destroyed, as may happen by a variety of causes, the ferruginous cellular texture that would remain would be very similar to the native iron of Siberia and South America; both which have, moreover, by chemical analysis, been found to contain an ample proportion of nickel. The Count, in describing the Siberian iron, mentions the hard transparent nodules found in the cells of that metal, which bear a great resemblance to the peridot (chrysolite of Werner), the decomposition of which leaves the empty cells which we observe in all the specimens that are brought to us. Mr. Howard, on analysing these transparent nodules,

found them consisting of the same elements as the earthy globules of the stone from Benares.

From a collective view of the contents of this paper, we may now reasonably infer, that all the substances here mentioned, not excepting the native irons from Siberia and South America, and some from Bohemia and Senegal, have a manifest relation with each other. Mr. Howard recapitulates these analogies; and, with a view to the further investigation of the subject, closes his paper with the following queries:—1st. Have not all fallen stones, and what are called Native Irons, the same origin?—2nd. Are all or any the produce of meteors?—and 3dly. Adverting to the circumstance of the Yorkshire stone having fallen during a serene sky, might not this stone have formed a meteor in regions too elevated to be within the reach of our observation?

Observations on the two lately discovered celestial Bodies. By William Herschel, *LL.D. F.R.S.* Read May 6, 1802. [*Phil. Trans.* 1802, *p.* 213.]

The observations described in the first part of this paper relate to the magnitude, the colour, the disc, and the atmosphere of the two moving stars lately discovered by Mr. Piazzi and Dr. Olbers, to which they have assigned the names of Ceres and Pallas; and also to the question, whether they be attended by any satellites. Their magnitudes were determined by means of a comparison of their reflected images with a lucid spot of a disc micrometer placed at considerable distances. The results of many observations were, that the diameter of Ceres does not subtend an angle of more than $0''\cdot40$, or in actual length only 162½ miles; and that the diameter of Pallas can hardly measure more than between 71 and 72 miles. The colour of these bodies was ruddy, and in one instance Pallas was of a dusky white. Their discs were never of a well defined planetary appearance; and as to their atmospheres or comas, the greatest extent of that attending Ceres was in one instance about two diameters beyond its disc; in another instance the whole had a cometary appearance; or when viewed most accurately, it bore a great resemblance to a small, much compressed, but ill-defined planetary nebula. Pallas never exhibited more than a faint haziness. As to satellites, it was inferred previous to the observations, that bodies of such very small dimensions could hardly contain a quantity of matter sufficient for the retention of secondary bodies; and in fact the several appearances that seemed to denote the existence of such attendants, were on more careful inspection found to be fallacious.

In the second part of the paper Dr. Herschel enters into an inquiry concerning the nature of these new stars, particularly as to the question whether they be planets or comets. And in order to proceed upon certain grounds, he previously enumerates certain criteria by which the heavenly bodies may be properly distinguished. These, as to the planets, are seven in number. 1. Those till lately discovered are all

of a considerable size; 2. They move in not very excentric ellipses round the sun; 3. The planes of their orbits do not deviate many degrees from the plane of the earth's orbit; 4. Their motion is direct; 5. They may have satellites or rings; 6. They have an atmosphere of considerable extent, which, however, bears hardly any sensible proportion to their diameters; and lastly, Their orbits are at certain considerable distances from each other.

These characters being next applied to the new stars, we find, in the first place, that as to the magnitude, they can certainly not be classed among planets; since the least of these (Mercury) is no less than 73,839 times larger than Pallas; 2. Their motions may perhaps agree sufficiently; but 3. The planes of the orbits of the new stars appear to deviate so much from that of the earth, that, were they admitted into the number of planets, we should have to extend the zodiac far beyond its present limits; and as it is not improbable that more of these bodies will be hereafter discovered, we shall perhaps end in having no zodiac at all. As to the 4th article, The motion of the new stars appears to be direct, like that of the planets. 5. Besides that no satellites have as yet been discovered belonging to these new stars, it may be inferred *à priori* that they have not a sufficient mass of matter to retain any. 6. The new stars, with respect to the small comas they exhibit, differ widely from planets, and might rather be classed among the comets, did not other circumstances militate against this opinion; and lastly, The orbits of the new stars are so near each other, that the admitting them as planets would materially affect the general harmony that takes place among the rest. As to their being comets, the five following criteria are here given, by which those desultory bodies have hitherto been distinguished:—1. They are generally of a very small size; 2. They move in very excentric ellipses, or apparently parabolic arches round the sun; 3. The direction of their motion is totally undetermined; 4. The planes of their orbits admit of the greatest variety in their situation; and 5. They have atmospheres of very great extent, which show themselves in various forms of tails, comas, haziness, &c. On applying these distinguishing characters to the new stars, we find, that as to size, they might perhaps be classed with comets; but that, as to the nature of the curve in which they move, the direction of their motion, and the planes of their orbits, they deviate still more from comets than they do from planets; and that, as to their atmosphere, though they bear in this respect a greater resemblance to comets than to planets, yet, upon a careful comparison with the appearances of several comets observed of late years, it seems evident that they are not entitled to a place among them; the most considerable of their comas being barely one-fiftieth part of the smallest that has ever been observed to attend a comet.

Thus it appears, that neither the appellation of planet nor comet can be assigned to these newly discovered bodies. And it remains, therefore, to find out some new term by which they may be distinguished. Previously to this, the author thinks it necessary to fix

upon an accurate definition, with a view to facilitate an appropriate denomination. According to this definition they are "Celestial bodies of a small or a very small size, which move in orbits of no very great excentricity round the sun, the planes of which may be inclined to the ecliptic in any angle whatsoever: their motion may be direct or retrograde; and they may or may not have very considerable atmospheres, or very small comas or nuclei."

Description of the Corundum Stone, and its Varieties, commonly known by the Names of Oriental Ruby, Sapphire, &c.; *with Observations on some other Mineral Substances. By the* Count de Bournon, *F.R.S.* Read March 25, 1802. [*Phil. Trans.* 1802, *p.* 233.]

In a former paper, printed in the Philosophical Transactions for the year 1798, Count de Bournon gave an analytical description of the crystalline forms of corundum from the East Indies and from China. From a note inserted at the beginning of the present communication, we learn, that the great number of specimens of that substance, since collected from different parts, chiefly of the East, have added so considerably to our knowledge relating to that subject, as to render it necessary not only to correct, but even, in many respects, to alter our opinion concerning it: and that hence, rather than create intricacy by introducing this additional knowledge in the form of a supplement, he had thought it expedient to collect all the information that could be obtained into one point of view, in hopes of delivering, in the present paper, with the addition of a chemical analysis which we are taught to expect from Mr. Chenevix, a complete mineralogical history of this curious substance.

The paper is prefaced by a short historical account of the opinions of former naturalists concerning the corundum stone, and its classification in the mineral system. The first of these, who derived their knowledge chiefly from lapidaries, had no hesitation in placing it among the gems, the hardest of which they distinguished by the epithet Oriental, and subdivided them according to their colours. Romé de Lisle was the first who deduced distinctive characters from the crystalline forms of the different sorts, rejecting the colour as a fallacious character. The first chemists who undertook to analyse this stone, thought themselves authorized to consider it as consisting of new elementary earths; but afterwards it was thought, and it appears now with much reason, to belong to the class of those stones which are chiefly, if not entirely, composed of argill. Werner at length also undertook the analysis; but he retrograded somewhat from what has been since found to be the truth, by placing it between pitch-stone and felspar. Abbé Hauy at length, recurring again to the crystalline form, placed it immediately after felspar, and before the Ceylonite; from both which, however, it differs widely, both by its hardness and specific gravity.

We are greatly indebted to the zeal and perseverance of our honourable member, Mr. Charles Greville, for a very ample collec-

tion of specimens of this stone, and of various other species which were long thought analogous, but are now found to constitute with it a genus of some extent. This substance, we are now told, presents itself to our senses, especially to our sight, under two very dissimilar appearances. The first is the original corundum imported under that name and also under that of adamantine spar. At times it is brought in irregular fragments, but often in crystals, which are generally of a pretty large size. Those of the other appearance are, in fact, the gems hitherto known by the names of Sapphire, Ruby, &c. Another distinction is next made, according to the texture or fineness of the grain of these stones; the former being called imperfect, and the latter, or the gems, perfect Corundum.

The author now enters into a minute account of the appearances and the principal properties of this substance. And first he treats of its *colour*. This, he tells us, is at best but an uncertain character in stones, but yet it may at times afford some secondary marks of distinction. The common or imperfect corundum varies, in this respect, according to the country where it is found. That from the Carnatic is of a grayish white, sometimes approaching to a pale green, and sometimes, though rarely, of a red or blue colour: that from China and Ava is generally of a dull green or brown colour; and that from the coast of Malabar appears of a reddish brown.

The perfect corundum which is found in Pegu and in Ceylon is either red, blue, or yellow. The former is the Oriental ruby; the blue is the sapphire; and the yellow the topaz. From a duly proportioned mixture of the blue and the red is produced the purple colour, which constitutes the Oriental amethyst. The union of the blue and yellow forms the green colour, which is proper to the Oriental emerald; and a larger proportion of yellow produces the colour proper to the chrysolite.

2. As to *transparency*, the crystals of corundum from the Carnatic, having usually rough surfaces, are of course very imperfectly diaphanous; but when broken, their fragments have generally a certain degree of semi-transparency: most of these fragments, when held up to the light, show a number of fissures in their substance, which in a great measure prevent the transmission of light. These fissures arise from a want of cohesion between all the parts of the crystalline laminæ. The red and blue corundum of the Carnatic has a greater degree of transparency than those of any other colour; the blue in particular having generally the preference in this respect.

3. In *hardness* the corundum comes next to the diamond; but this quality, with regard to intensity, differs greatly, according to the colour and degrees of purity of the specimens. The corundum of the Carnatic, when it is neither blue nor red, is less hard than any other sort; whilst the imperfect blue species exceeds all the other varieties of this kind in hardness,—such is its density that it will emit pretty bright sparks when struck with steel.

4. This substance, like quartz, becomes phosphorescent by collision. The crystals of a red colour, whether of the perfect or im-

perfect kind, will emit a light of a very deep fire-colour, similar to that of a red hot iron.

5. The specific gravity of this stone varies in all its different kinds. The means deduced from a great number of observations afford the following numbers.

Imperfect corundum 3931. Perfect corundum, in the instance of Oriental ruby, 3977; and of sapphire, 4158. The difference seems to be proportionate to the degree of perfection of the crystallization, and consequently of the transparency of the stone.

6. We come now to the most extensive and most elaborate section of the paper which treats of the *crystalline forms* of the different kinds of corundum. The primitive form of all the kinds, whatever be their degree of perfection, we are here told is a *rhomboid* slightly acute, the obtuse angles of the planes measuring 94°, and the acute ones 86°; and it is asserted, that whatever the form of an individual crystal may be, it may always, by dividing it according to the lately established rules of crystallography, be ultimately reduced to this rhomboidal form. The manner in which crystals deviate from their primitive form, by the substitution of planes for the angles, effected by the retreat of rows of molecules, which constitute the crystalline laminæ, is amply discussed in a note; and nine modifications are described, forming a great variety of prismatic, pyramidal, and other crystals, of which some idea can only be formed by an inspection of the figures that accompany the paper.

7. The next section treats of the *fracture and texture* of this stone. In general we are told that all the kinds have a lamellated texture, the layers being in a direction parallel to the faces of the rhomboid, and that they break in a direction parallel to those faces. The ease, however, with which these laminæ may be divided, differs greatly in the different varieties; and this is ascribed to the degree of force existing in the attraction of the molecules which compose these crystals, as well as to the perfect adhesion of the crystalline laminæ composed of those molecules at all points of their surface. This attraction and adhesion, it is thought, varies with the colour of the stone, the blue or sapphire possessing those qualities in the highest degree, which accounts for the fracture of this stone being often in a direction oblique, and even at right angles to the planes of the laminæ.

8. The 8th section contains some observations on the phænomena of light exhibited by this stone. The prismatic, as well as the pyramidal crystals of corundum, when their extremities are terminated by planes which are perpendicular to their axes, very frequently exhibit on these planes a changeable variety of colours, known by the name of *chatoyant*. This property is ascribed to the reflection of light in the small intervals which remain between the crystalline laminæ in those parts where these laminæ are not in perfect contact. It follows hence that the most compact sorts of corundum will not exhibit this appearance. To the same property is also ascribed that beautiful reflection of light in the form of a star of six rays, frequently pro-

duced in rubies and sapphires when cut in a particular direction. As to the manner of cutting these asteries, or star-stones, as they are usually called, it appears to be rather the effect of chance than of any determined theory. Some hints, however, are here given for the purpose, which chiefly recommend a proper attention to the primitive rhomboidal form of the crystal.

In the 9th section the author treats of the characters of the corundum afforded by chemical analysis; and here he anticipates the results of the investigation we are to be favoured with by Mr. Chenevix. These confirm what he has all along maintained in this paper of the identity of the several kinds of stones which he has classed under the name of Corundum.

The ingredients are uniformly found to be the same in all the species, differing only, and but in a small degree, in their proportions. The principal of these ingredients is argill or alumina, which, in the imperfect corundum from the Carnatic, Malabar, China, and Ava, consist of between 86½ and 91 in 100 parts. In the sapphire it amounts to 92, and in the ruby to 90 hundredths. The other constituent parts are silica and iron; the former in the greatest proportion. Of the identity of stones which bear very different appearances, another example is here given in the instance of the felspar, reasons being assigned why some species of schorl, the adularia, and some other substances, ought to be classed with it.

The author proceeds next to consider the sort of corundum which does not exhibit the smallest rudiments of crystallization, and which mineralogists have agreed to distinguish by the name of *Compact Corundum*. It resembles, in many respects, a coarse jasper; but its much greater degree of hardness, and its much higher specific gravity, render its true nature easily distinguishable. It has a lamellated appearance. The red sort, in particular, gives pretty strong sparks when struck with steel. It is phosphorescent, like crystallized corundum. Its specific gravity is 3902.

Three sections treat next of the matrices of different sorts of corundum; which lead the author to dwell largely on a variety of substances which hitherto were thought to have no kind of affinity with this stone.

The first section treats of the matrix of imperfect corundum from the peninsula of India, chiefly from the Carnatic, and of the substances with which it is accompanied. This matrix, as far as our present knowledge extends, appears to be a stone of a particular nature; sometimes of a loose granulated texture, not unlike a coarse sandstone; and at other times of a closer grain, similar to the kind of marble known by the name of Coarse-grained Saline Marble: both kinds are of a pearly gray colour, sometimes slightly tinged with green, and have a degree of semi-transparency not unlike chalcedony. Their specific gravities are inferior to that of felspar. In this substance the crystals of corundum are imbedded, nearly in the same manner as those of felspar are dispersed in porphyry or certain granites. The accompanying substances are, 1. *Lamellated fragments*, not un-

like felspar or adularia, and partaking of many of the properties of corundum itself, and even of its crystallization. 2. *Fibrolites*, which are described as small masses frequently crystallized, but different in some respects from all other mineral substances hitherto known. 3. *Thallite*, or the Epidote of Abbé Hauy. This is minutely described in three different states. 4. *Hornblende*, which is most constantly and most abundantly contained in the matrix here treated of. 5. *Quartz, Talc, Mica, Steatite, Garnets, Zircon*; all which, though manifestly dispersed in this matrix, are yet less frequent. And lastly, the presence of *black Oxide of Iron* is likewise evident, though not in such large proportions as in the matrix of the imperfect corundum from China.

Section 2. Of the matrix of imperfect corundum from China, and the substances with which it is accompanied.—This matrix is totally different from the preceding one, being a granite rock, composed of an aggregate mixture of felspar, fibrolite, mica, and attractable black oxide of iron: but none of that particular substance which has been mentioned as forming the basis of the preceding matrix is here observed. The four substances above mentioned are unequally distributed throughout the mass; some pieces being composed almost entirely of only one of them, while in other pieces the substances are mixed together in different proportions, and sometimes in nearly equal ones.

Section 3. Of the matrix of perfect corundum from the Island of Ceylon, and the substances of which it appears to be composed.—The author, on this head, speaks with some diffidence, as the precious stones comprised under the denomination of this kind of corundum are selected by the inhabitants from the sands washed down by the rivers or rivulets of the island, and have seldom been brought to Europe in any kind of matrix. He gives us, however, a list of the substances which compose the sands that are sent to us from Ceylon; although he will not venture to assert positively that these substances really accompany the corundum when in its matrix. They are, 1. *Spinelle Ruby*, which generally composes nine parts in ten of the whole mass of this sand, but in such small crystals or fragments as to render them of little or no consequence in trade; owing, no doubt, to the selection made in India before it is sent over. The few of a tolerable size that have been obtained, are here described as to their crystalline form, their colours, their peculiar matrix, in some of which was found an iron ore hitherto unknown, while some of them appeared evidently to be masses of adularia. 2. *Tourmalin*, in the form of a very obtuse rhomboid, with several varieties deducible from this form, and of different colours. 3. *Ceylonite*, so called by M. la Matherie, but distinguished by the name of Pleonaste in the Mineralogy of the Abbé Hauy. This is likewise of a variety of crystalline forms and colours. 4. *Zircon*, which, next to the Spinelle, is the substance most frequently found in the sand of Ceylon, the crystals of which, though very small, are yet in general very perfect. And lastly, though their numbers be very insignificant, some small scattered

fragments of quartz, felspar, calcareous spar, a brownish-yellow mica, and particles of attractable oxide of iron.

The paper closes with some account of corundum, which, contrary to the received opinion, that this stone was only found in the East Indies, has been thought to exist in other parts of the world. The author dwells mostly upon the appearances of a stone he himself discovered in the mountainous parts of the Forez in France, and which the Abbé Hauy considers only as a species of felspar. The Count alleges his reasons for classing it with the perfect blue corundum, known by the name of Sapphire. As to various stones found in Germany, in the Isle of Tirree on the western coast of Scotland, on Chesnut-hill near Philadelphia, and elsewhere, which have by some been considered as corundum, the author cautions us against acquiescing in those assertions till more conclusive arguments shall appear in their favour.

Analysis of Corundum, and of some of the Substances which accompany it; with Observations on the Affinities which the Earths have been supposed to have for each other, in the humid Way. By Richard Chenevix, *Esq. F.R.S. and M.R.I.A.* Read May 20, 1802. [*Phil. Trans.* 1802, *p.* 327.]

After a detail of several unsuccessful attempts to analyse this stone, which on account of its great hardness is both difficult to pulverize and to be reduced by saline agents, we find an ample description of the process, which was attended with the desired success. A piece of corundum, weighing 100 grains, was made several times red hot, and plunged into cold water; it was then pounded, first in a steel, and next in an agate mortar, and thus reduced into an impalpable powder. This powder was by means of dilute muriatic acid cleared from the ferruginous particles which adhered to it from the steel mortar. It was then put into a platina crucible with 200 grains of sub-borate of soda, and the mixture was exposed for an hour or two to a violent heat: the glass produced by this fusion was in about twelve hours dissolved, by boiling it in a proper quantity of muriatic acid.

The silica might now have been separated by evaporating the whole to dryness, but it was thought preferable to get rid of all the salts contained in the liquor by a precipitation effected by means of an alkaline carbonate. The precipitate thus obtained was then redissolved in muriatic acid, and the silica was hence cleared by evaporation. The remaining liquor was afterwards boiled with potash, by which means the alumina was precipitated. It was then redissolved by the excess of potash, from which the earth was finally obtained by muriate of ammonia. A small proportion of iron was separated by muriatic acid. Both these earths being now washed and dried, were ignited, and thus the exact weight of each was accurately ascertained. The author paid particular attention to the silica produced in this process; as Mr. Klaproth, who had formerly analysed this stone, declares that he never found any of this ingredient.

Next follow the tables of the contents of six kinds of corundum, viz. the sapphire, the ruby, and the corundum from the Carnatic, from Malabar, from China, and from Ava. The proportions of the first species are $5\frac{1}{4}$ silica, 92 alumina, 1 iron, and $1\frac{3}{4}$ loss. The proportions of the other kinds do not differ very considerably from these.

The matrices of these stones being more easily fused than the six kinds above mentioned, the usual and well known mode of treatment by potash was found sufficient to render them soluble in acids. Although this mode be now very familiar to chemists, the author, however, in order to leave no cause for suspicion, describes the process he used with the matrix of the corundum from the peninsula of India. The results gave $42\frac{1}{2}$ silica, $37\frac{1}{2}$ alumina, 15 lime, 3 iron, and 2 loss, with a trace of manganese. By similar treatment the various substances contained in this and some other matrices, viz. felspar, fibrolite, and three sorts of thallite, were analysed, and the results are given in tables. It is remarkable that while all the other substances yield in different proportions the same ingredients as the matrices themselves, the fibrolite was found to consist only of silica and alumina, the quantity of iron it contained being so small as hardly to deserve notice.

In the prosecution of this inquiry Mr. Chenevix observed, that if a quantity of potash be for some time kept in fusion in a platina crucible, the latter will be found to lose some grains of its weight. The quantity of the metal thus lost he actually found in the potash; and hence he infers the affinity between these two substances, which affinity, it seems, is made use of by the Spaniards for detecting the platina contained in the ingots of gold sent from their American possessions. He also has occasion to show that potash which has usually been denominated a fixed alkali is not so, strictly speaking, since there is a degree of heat by which it may be totally volatilized.

In a second part of the paper the author treats of the affinities which the earths are supposed to exercise towards each other when held in solution by acid or alkaline menstrua. There being a difference of opinion on this subject among some of the most eminent chemists, Mr. Chenevix has repeated many of their experiments, especially those of Guyton de Morveau. After descanting largely on the probable causes of error in this eminent chemist, as well as in Mr. Kirwan and others, he derives from his results the following general conclusions.

1st. That there exists an affinity between silica and alumina.

2ndly. That there exists a very powerful affinity between alumina and magnesia.

3rdly. That alumina shows an affinity for lime.

4thly. That Mr. Guyton was mistaken in every instance of affinity between the earths, except in one which had been observed before his experiments; and that he has attributed to a cause which does not exist, phænomena that must have resulted from the impurity of his re-agents.

And lastly, That neither the experiments of Mr. Guyton, nor an

opinion maintained in an anonymous letter from Freyberg, published in the 4th volume of Mr. Nicholson's Journal, are sufficient to diminish in any degree the value of that assistance which mineralogy derives from chemical investigation.

Description of the Anatomy of the Ornithorhynchus Hystrix. *By* Everard Home, *Esq. F.R.S.* Read June 3, 1802. [*Phil. Trans.* 1802, *p.* 348.]

The specimen from which this description was taken, and which was exhibited to the Society at their Meeting, was brought from New South Wales. It is a male, probably arrived at its full growth. It is seventeen inches in length from the point of the bill to the extremity of the tail; and its greatest circumference measures likewise about seventeen inches. Its back and sides are covered with quills, the longest of which are about two inches and a half in length. Its bill projects from the head one inch and three-fourths, tapering from its base, where it is seven-eighths of an inch in diameter to its point, where its diameter is not above three-eighths of an inch. It is tubular, convex on the upper, and flat on the lower surface. The tongue is cylindrical, very small towards the point, and eight inches long. This species has a peculiarity in its mode of managing its food, which distinguishes it from the Paradoxus. The food is first bruised by small horny prominences adhering to the tongue and palate, and then swallowed with a certain quantity of sand, the stomach being sufficiently large to contain this extraneous matter, together with the food, and effectually defended from injury by a cuticular lining.

Mr. Home proceeds, with his usual accuracy and minuteness, in his technical description, both of the external and internal parts, which he illustrates with a number of figures. Having completed this detail, he observes in general, that this species of Ornithorhynchus being a nearer approach to the more perfect quadruped than the Paradoxus, and its tongue being in some respects similar to those of the Manis and Myrmecophaga, he thought it necessary to look among the different species of these genera for other parts of resemblance. The result of this comparison is, that the Ornithorhynchus is essentially different from all other quadrupeds, bearing in some respects a resemblance to birds, and in others to the Amphibia, so that it may be considered as an intermediate link between the classes Mammalia, Aves, and Amphibia. To the first class it no doubt approaches nearest in the instance of the Myrmecophaga; and to the birds it bears a singular affinity in the male organs of generation, as is here illustrated by comparing its penis with that of the drake.

From the whole of this investigation are deduced the following characters as peculiar to this animal, considered as a genus. The male has a spur on the two hind legs, close to the heel. The female has no nipples, differing essentially in this, as well as in the organs of generation, from the Mammalia. The beak is smooth, while the rest of the animal is covered with hair. The tongue has horny pro-

cesses, which answer the purposes of teeth. The penis of the male is solely appropriated to the passage of the semen, its external orifice being subdivided into several openings, so as to scatter the semen over an extent of surface, while the urine passes by a separate canal into the rectum. And lastly, the female has no common uterus, the tubes, which correspond to the horns of the uterus in other quadrupeds, receiving the semen immediately from the penis of the male.

Mr. Home concludes his paper with a conjecture, that more species of this extraordinary animal will be gradually discovered; a drawing having already been received from Van Diemen's Land of an individual similar to the Hystrix here described, only with the spines much shorter. And he thinks it probable that the class will in time be found to consist of various kinds, and that those hitherto known will be arranged under different genera.

A Method of examining refractive and dispersive Powers, by prismatic Reflection. By William Hyde Wollaston, *M.D. F.R.S.* Read June 24, 1802. [*Phil. Trans.* 1802, *p.* 365.]

The principle of this method depends on the reflection of light at the inner surface of a denser refracting medium. Its application in the first instance is deduced from a theorem, from which we gather, that since the range of inclination within which total reflection takes place, depends not only on the density of the reflecting prism, but also on the rarity of the medium adjacent to it, the extent of that range will vary according to the difference of the densities of the two media. And that hence when the refractive power of one medium is known, that of the rarer medium may be thence inferred, by ascertaining the angle at which the ray of light will be reflected from it.

Having exemplified this by several instances of different media, the author proceeds to give an account of an instrument he has contrived for the purpose of applying this principle to practice. Its object is to measure by mechanical means, which can only be understood by inspecting the drawing annexed to the paper, the sine representing the refractive power of the medium applied to the prism. One of the advantages which this method possesses above the usual mode of examining refractive powers is, that whereas hitherto it was necessary that the substances under examination should have two surfaces inclined towards each other under a known angle, this method enables us to deduce the results from only one surface, and without any computation. Hence also, as trials can be made by mere contact, may the refractive powers of opake bodies be easily determined. And these powers in different bodies may likewise be used as convenient tests in many philosophical inquiries.

This method applies also to media of which the refractive densities are not uniform, such as the crystalline lens in the eyes of animals, which is now known to be more dense in the centre than at its surface. It is here shown in what manner, by placing one of these varied media in contact with a prism, all its gradations of density,

from greatest to least, will become at once manifest by mere inspection. Lastly, a table is given containing a series of substances arranged according to their refractive powers, chiefly deduced from experiments made according to the method here described; some to which the machine for measurement would not apply, being obtained by other means, or borrowed from other authors.

The second part of the paper treats of the dispersion of light. The principles and observations on which the inductions here given chiefly depend, are these:—When a glass prism is placed in contact with water, and brought near the eye, in such a position that it reflects the light from the window, the extent of perfect reflection is seen to be bounded by a fringe of the prismatic colours in the order of their refrangibility. But it may happen that two media which *refract unequally* at the same incidence, may *disperse equally* at that incidence; and, under these circumstances, a pencil of rays passing from one of these media into the other, will be refracted without dispersion of its colours. The boundary of prismatic reflection will then be found a well defined line, free from colour, if the surface at which the reflected light emerges from the prism be at right angles to its course. Moreover, when the disparity of the dispersive powers of the media is still greater, it may also happen that the usual order of prismatic colours will be reversed, and then the red, or least refrangible ray, will appear strongest and lowest in the fringe, unless the colours so produced are counteracted by refraction at their emergence from the prism.

This doctrine is illustrated by examples of various, both simple and compound, substances, and especially by the effect of metallic solutions differently diluted in less dispersive media. Having compared several of these, each diluted till the limit of reflection appeared void of colour when in contact with a rectangular piece of plate-glass, he deduced thence a table of their refractive powers, in that state of dilution in which the eye could discern the disappearance of colour.

He likewise made experiments on dispersion by means of wedges, in the manner practised by Mr. Dollond, Dr. Blair, and others; and has reduced the substances thus examined into a second table, arranged according to the excess of their effect on violet above red light, at a given angle of deviation. By comparing this with the preceding table, it appears how little correspondence there is between them, and consequently how numerous are the combinations by means of which a pencil of rays that passes through two media, may be made to deviate without dispersion of its colours.

At the close of this paper the author remarks, that the colours into which a beam of white light is separable by refraction, appear to him to be neither seven, as they are usually seen in the rainbow, nor reducible to three, as some persons have conceived; but that by employing a very narrow pencil of light, four primary divisions of the prismatic spectrum may be seen, with a degree of distinctness which, he believes, has never been described nor observed before. These colours are red, yellowish green, blue, and violet, in the proportion nearly as the numbers 16, 23, 36, 25.

On the oblique Refraction of Iceland Crystal. By William Hyde Wollaston, *M.D. F.R.S.* Read June 24, 1802. [*Phil. Trans.* 1802, *p.* 381.]

In the preceding communication Dr. Wollaston inserted two different measures of refractive powers distinctly observable in the Iceland crystal, as well as an estimate of its dispersive power; but he has reserved for this treatise some remarks, which the same mode of investigation has enabled him to make on its oblique refraction. To this he was led by the consideration that the law to which Huygens had reduced this refraction, however founded in truth, could not be easily verified by any of the former methods of measurement.

According to the Huygenian hypothesis, light proceeding from any luminous centre is propagated by vibrations of a medium highly elastic, that pervades all space. In ordinary cases the incipient undulations are of a spherical form; but in the Iceland crystal they appeared to him to be portions of an oblate spheroid, of which the axis is parallel to the short diagonal of an equilateral piece of crystal, and its centre the point of incidence of the ray. Hence he deduced a ratio between the sine of incidence, and the sine of refraction (that is, the ordinate of the spheroidical undulation) in any section of the spheroid.

In a geometrical deduction our author shows that his observations on this substance accord throughout with the hypothesis of Huygens, the measures he has taken corresponding more nearly than could well happen in case of a false theory. This is illustrated by various examples, in which the refractive power is estimated according to various directions of the plane of incidence; and the data are pointed out for the construction of the spheroid, by which these refractions are regulated. Lastly, a comparative view of the angles observed, and those obtained by computation, is reduced into a table, from which, by their near agreement, we collect an additional proof of the accuracy of the results.

An Account of some Cases of the Production of Colours, not hitherto described. By Thomas Young, *M.D. F.R.S. F.L.S. Professor of Natural Philosophy in the Royal Institution.* Read July 1, 1802. [*Phil. Trans.* 1802, *p.* 387.]

In a former paper Dr. Young, treating of certain phænomena of coloured light, mentioned a law, according to which it appears, that whenever two portions of the same light arrive at the eye by different routes, either exactly or very nearly in the same direction, the light becomes most intense when the difference of the routes is any multiple of a certain length, and least intense in the intermediate state of the interfering portions, and that this length is different for light of different colours. In the same paper he showed the sufficiency of this law for explaining all the phænomena in the second and third books of Newton's Optics; and in the present communication he il-

lustrates it still further, by applying it to some new distinct cases relating to the colours of fibres, and to the colours of mixed plates.

The case respecting fibres is that of the coloured fringes produced by the interposition of a hair between the luminous object and the eye. Here it is observed that the fringes are larger and brighter in proportion as the hair is thinner, the phænomenon being most conspicuous when a single thread of a silkworm is interposed. The cause here assigned is the interference of two portions of light, one reflected from the fibre, and the other bending round its opposite side, and at last coinciding nearly in direction with the former portion; hence as both portions deviate more from a rectilinear direction, the difference of the length of their paths will be gradually increased, and consequently produce the appearances of colour usual in such cases. The rule given to calculate the difference of the paths for the light least inflected, is the analogy between that difference to the diameter of the fibre, which will be as the deviation of the ray at any point from the rectilinear direction, to its distance from the fibre.

When a number of fibres of the same kind,—as, for instance, an uniform lock of wool,—are held near to the eye, we see an appearance of halos surrounding a distant candle; but their brilliancy, and even their existence, depends on the uniformity of the dimensions of the fibres, and they are larger as the fibres are smaller. To an effect similar to this are ascribed the coloured atmospherical halos, substituting to the above fibres a number of particles of water of equal dimensions, and properly situated between the luminary and the eye.

Speaking of the colours of mixed plates in looking at a candle through two pieces of plate-glass with a little moisture between them, we are told that the fringes here produced are the effect of moisture, intermixed with portions of air, exhibiting an appearance similar to dew. Here the light transmitted through the water, moving in it with a velocity different from that of the light passing through the interstices filled only with air, the two portions, it is said, will interfere with each other, and produce the effect of colours according to the general law.

In further applying this general law, the author found it impossible to avoid another supposition, which is a part of the undulatory theory he defended in his former paper; namely, that the velocity of light is the greater the rarer the medium: and he suggests an idea, which appears to him to lead to an explanation of the dispersion of colours by refraction, more simple and satisfactory than that which he formerly advanced. He supposes that every refractive medium transmits the undulations constituting light in two separate portions, one passing through its ultimate particles, and the other through its pores; and that these portions re-unite continually after each successive separation, the one having preceded the other by a very minute but constant interval, depending on the regular arrangement of the particles of a homogeneous medium. Having briefly discoursed on the application of this doctrine, he concludes by mentioning some

observations which appear to him to confirm what he had advanced on a former occasion touching the dispersive powers of the eye.

On the Composition of Emery. By Smithson Tennant, *Esq. F.R.S.* Read July 1, 1802. [*Phil. Trans.* 1802, *p.* 398.]

The ultimate results of the experiments made on this substance, which it seems had never before been properly analysed, are—that 25 grains contain 12½ grains of argillaceous earth, 2 of silex, and 8 of iron; that 1 grain was not dissolved, and that the remainder, being 1½ grain, was lost in the process. Another process gave the same components, but in somewhat different proportions. These ingredients being very similar to those found by Mr. Klaproth in Diamond spar, it is thought that emery is essentially a substance of the same nature, with perhaps a somewhat greater proportion of iron.

Quelques Remarques sur la Chaleur, et sur l'Action des Corps qui l'interceptent. Par P. Prevost, *Professeur de Philosophie à Genève, &c. Communicated by* Tnomas Young, *M.D. F.R.S.* Read July 1, 1802. [*Phil. Trans.* 1802, *p.* 403.]

The remarks here brought forward relate chiefly to Dr. Herschel's experiments on the solar and terrestrial rays that occasion heat, published in the Philosophical Transactions for the year 1800, and are meant to rectify some anomalies which appear in their results. The paper consists of two parts: the first being the observations on Dr. Herschel's experiments, and some new ones, with inferences deduced from them; and the second the exposition of a theory, which the author thinks may reconcile all contradictions.

In the first part he sets out with briefly stating the manner in which Dr. Herschel conducted the experiments he made, in order to estimate, by the indications of different thermometers, the quantity of heat transmitted through various substances, compared with the heat afforded by direct rays from different luminous bodies, or more properly sources of heat. Here the author soon starts a difficulty concerning the mode of estimating the intercepting power of the substances used in the experiments. As these experiments consist of a series of observations made progressively at intervals of one minute between each other, it follows that the ratio Dr. Herschel adopted between the heat produced by direct rays, and those transmitted through coloured media, is not, as he imagined, a constant proportion, he having uniformly deduced his inferences from the differences between the initial and the final degrees of heat; whereas, had he attended to the intermediate observations, he would have found that each of them would have afforded a different ratio.

Having maturely considered this subject, the author, adverting to this circumstance of the various proportions of heat progressively yielded in these experiments, observes, that it can hardly be conceived why the faculty of transmitting and intercepting heat should

thus vary in any substance, merely because it has transmitted or intercepted it for a greater or less length of time. Hence he thinks it essential to have recourse to some permanent rule from which the results may in all cases be accurately derived, and which, when the phænomena do not correspond, may lead us to the investigation of some other cause. Such a law has been deduced from direct experiments, and implies that a body placed in a medium of a constant temperature, becomes heated or cooled in such a manner, that the differences of its heat from that of the medium are in a geometrical progression, while the times of heating or of cooling are arithmetically proportionate. It will readily be perceived in what manner it is practicable to deduce from the two progressions mentioned in this law, a third progression, which will apply to the intermediate steps of any series of observations.

This law, when adapted both to Dr. Herschel's experiments and to some new ones here described, is found to apply with singular accuracy through the three or four first minutes of increasing heat; but after this period the series manifestly varies, the increase of heat by computation according to the law falling progressively short of that indicated by the thermometers. The author is at considerable pains to explain this anomaly, and at length ascribes it to the heat accumulated in the intercepting body, which renders it in a manner a new source of heat, the emanation from which, it must be admitted, cannot but cooperate with the transmitted rays, to raise the thermometers near it.

If the progress of this accumulation of heat be perfectly regular, its effect will be confounded with that of the transmitted rays, as was actually found to be the case when a thin plate of talc was used as an intercepting medium. The cause of this difference is ascribed chiefly to the thickness of that medium, and in some measure also to the weakness of the source of heat. It will scarcely be necessary to explain the operation of these concurrent causes, it being obvious that the greater the bulk of a body, the greater will be the accumulation it admits of, and the greater the source of heat, the more rapid will be this accumulation.

The next object of inquiry is how long an experiment should last for the thermometer to acquire the maximum of heating, that is, the temperature of the source of heat, or medium in which it is immersed. Here the experiments can be made only on direct heat, since the intermediate body containing accumulated heat, might, and probably does in most cases, continue to emit this heat after the thermometer has arrived at the maximum, that is, the temperature of the source of heat. In the direct heat of the sun this maximum was obtained in little more than 12′.

The author hereupon examines a number of Dr. Herschel's experiments, in which he mentions only the initial and final degrees of the thermometer. After showing what the mean ratio is between the degrees computed for the progression of the differences, and those determined by observation, which he finds is as 13 to 10, he deter-

mines the constant heat of a medium by the following proportion. The difference between this heat, and each of the numbers given by observation (that is, the initial and final observation), are to each other as the first term of the progression is to the sixth; that is to say, as the numbers 13 and 10 raised to the fifth power.

These comparisons between his results and those Dr. Herschel had derived from the same experiments, have led our author to several remarks, in which the above-mentioned law, and the circumstance of the accumulation of heat in the intercepting media, are applied to various phænomena and computations, and likewise to some experiments of the same nature described by Prof. Pictet in his Essay on Fire. The deviations here observed are in most cases ascribed to the thickness of the intercepting substances, and to the distances between them and the thermometers.

The second part, which relates to the theory from which depends the law of the increments of heat, as deduced from direct observations, is introduced by a brief statement of the historical facts that have led to the contemplation of this subject. Bacon first proposed the question, whether heated bodies, which are obscure and opake, are similar in their effects to the radiant bodies? Several philosophers, such as Lambert, Saussure, and Pictet, have by various experiments determined in favour of the affirmative; and it has even been proved that the velocity of heat, independent of light, is no less than 69 feet in an instant of time not apparently divisible.

Bacon likewise asked whether cold might not, as well as heat, acquire intensity by means of mirrors or refracting glasses? Our author, without mentioning the well-known experiments of the Academy del Cimento on this subject, proceeds at once to those of Prof. Pictet, who proved the affirmative as to the fact, but yet thought that the cause ought to be ascribed not to the reflected cold, but to the reflection of heat in opposite circumstances; by which he seems to understand that heat in this instance escapes reciprocally from the thermometer towards the cooler substance. He here substitutes a moveable equilibrium, to the immoveable one usually admitted by philosophers; and this he thinks fully explains the identity of the phænomena according to his theory, which implies an equal apparent dispersion of heat and cold.

This theory is as follows:—Fire is a discrete and agitated fluid; every molecule of free fire is moved with great velocity: some molecules move one way, some another, so that a hot body throws out calorific rays in every direction. And these molecules have sufficient distance between them to admit two or more currents to cross each other without being impeded in their course. This character of fire being clearly understood, it must be evident (says our author) that if we suppose two neighbouring spaces to contain a certain quantity of it, there must be continual changes between them. If the fire is equally abundant in each, the changes will be equal, and an equilibrium will be produced: if one of the spaces contain more fire than the other, the changes will be unequal; but after a sufficient time

the continual repetition of these changes will likewise produce an equilibrium. According to these principles he undertakes to explain all the laws of increasing and decreasing heat; he supplies us with various examples as to the application of them, and shows how this theory coincides with the general law laid down in the first part of the paper.

The drift of this treatise will be still further illustrated, if we attend to the following recapitulation given us by the author of the leading principles deduced from the various parts of his investigation.

1. The effect of a constant source of heat upon the thermometer is not proportional to the heat of that source.

2. We nevertheless possess a method of determining the heat of the source by its effect on the thermometer, because we know the law this effect follows in its successive increments.

3. This method is the only one that ought to be employed when it is required to compare two sources of heat, according to their effect in a limited time, less than that which is necessary to produce the maximum of the effect.

4. In the case of transmitted heat, we must distinguish that which is immediately transmitted, from that which is added by the transmitting body after it becomes heated.

5. If we neglect making the distinction, the interception of heat attributed to the intercepting body is only an inferior limit or minimum; so that it remains undetermined whether the interception has not been much greater, or even total.

6. By applying these principles to Dr. Herschel's experiments, a more exact appreciation may be obtained; it is, however, governed by some accessory circumstances, which have not yet been determined.

7. In those experiments the apparent difference between the interception of heat and of light by the same substances, does not afford any fair conclusion respecting the difference or the identity of light and heat.

8. The law mentioned in the first part of the paper is not only proved by direct experiments, but also by its agreement with the true theory of the earth.

Lastly. This theory is established upon various facts, entirely different from the above law, and it is the only one which agrees with the general phænomena of nature.

Of the Rectification of the Conic Sections. By the Rev. John Hellins, *B.D. F.R.S. and Vicar of Potter's-Pury, in Northamptonshire.* Read July 8, 1802. [*Phil. Trans.* 1802, *p.* 448.]

This, it is to be observed, is only the first part of a more extensive work, and relates merely to the rectification of the hyperbola. After a few strictures on the necessity of not relaxing in our endeavours to improve the method of fluxions, to which the author asserts few additions have been made since its first discovery by the immortal

Newton, he informs us of the circumstances which led him to the present investigation, namely, the occasion he had some years ago to solve a problem which required the rectification of an equilateral hyperbola.

He then enters upon his subject; and in a first section he investigates in nine theorems the several series which apply to this curve, whose different characters, namely, the ratios of their terms, or rather the rates of their convergency and divergency, depend on the relative proportions of their elements. Of these series one only, and that not the best, is all that he has hitherto been able to find in other works. Two are of the form which is called ascending, and six descending. One of them is of a peculiar form, which can only be understood by turning to the paper. Among these series, he observes, may always be found some which will converge, whether the portion of the hyperbolic arch taken from the vertex be long or short, or of a moderate length; but the ascending series always differs from the descending one by a constant quantity.

In a second section the author treats of the methods of computing the values of the constant quantities, by which the ascending series differ from the descending ones. Here he has recourse to two methods, of which he has already given an illustration in his Mathematical Essays: the one by computing the value of both an ascending and descending series, taking for the ordinate to the axis some small definite quantity; and the other by comparing the values of those series together, when the ordinate is taken immensely great. The former method he says is more general; but the latter, when it can be applied, usually affords the easiest computation.

In the third section are given five examples, which show the use of these theorems, as well as the manner of choosing such as are best adapted to any particular case. In one of these the author corrects an error in the length of a large arch of an equilateral hyperbola, which was first published in the year 1771, and has been since reprinted by some eminent mathematicians.

Lastly, he concludes with some remarks on former writers, and takes notice of the defects of two series given by the late Dr. Waring for the rectification of an hyperbola.

Catalogue of 500 *new Nebulæ, nebulous Stars, planetary Nebulæ, and Clusters of Stars; with Remarks on the Construction of the Heavens. By* William Herschel, *LL.D. F.R.S.* Read July 1, 1802. [*Phil. Trans.* 1802, *p.* 477.]

To this catalogue is prefixed a classification of the multitude of sidereal bodies hitherto discovered, not according to their apparent magnitudes or appearances on our earth, but according to their peculiar nature and arrangement in the heavens. They are divided into the twelve following classes:

1. *Insulated stars*, or such as may be considered out of the reach of mutual attraction; such as our Sun, Arcturus, Capella, Lyra, Si-

rius, &c.—These indeed, as well as all other heavenly bodies, cannot be said to be entirely free from the influence of the stars surrounding them; but the character assigned to them is, that the attraction in one direction is so counteracted by a contrary influence of the same nature, as to be retained for many ages in a state almost equal to undisturbed rest. Dr. Herschel suspects that we are to look for solar systems only among those insulated stars.

2. *Binary sidereal systems*, or double stars.—It is sufficiently obvious that these are not stars seen nearly in the same visual ray, for these rays may be an immense distance from each other; but by these are meant two stars that are connected together by the influence of attraction. It is easy to prove, by the doctrine of gravitation, that two stars thus connected, and sufficiently distant from the influence of other celestial bodies, will perform revolutions round a common centre of motion; that hence they will always move in directions opposite and parallel to each other; and that their system, if not destroyed by some foreign cause, will remain permanent. This kind of rotation is exemplified by the instance of our earth and the moon. Dr. Herschel proposes, on a future occasion, to communicate a series of observations made on double stars, whereby it will be seen that many of them have actually changed their situation with regard to each other, in a progressive course, denoting a periodical revolution round each other, and that the motion of some of them is direct, while that of others is retrograde.

3. *More complicated sidereal systems*, or treble, quadruple, and multiple stars.—From the combination of two stars, it is easy to advance a step further, and allow that three or more stars may be connected in one mutual system of reciprocal attraction; and the computation for determining the common centre of their respective orbits is here exemplified by a variety of hypothetical cases. The author at the same time asserts, that there is not a single night when in passing over the zones of the heavens by sweeping, he does not meet with numerous collections of such multiple stars, apparently insulated from other groups, and probably joined in some small sidereal system of their own.

4. *Clustering stars.*—These are described as great collections of small stars that are profusely scattered over the milky way, by no means uniformly, but unequally dispersed in many separate allotments. An instance of one of these aggregates is given, which in a space of about 5° between β and γ Cygni, contains above 331,000 stars. A more particular account of the milky way, we are promised, will be the subject of a future communication.

5. *Groups of stars.*—These differ from the preceding class by being collections of closely, and almost equally compressed stars, of any figure or outline; and from the next following, by showing no particular condensation that seems to point out any ideal centre of attraction.

6. *Clusters of stars.*—These are generally round, and the compression of their stars indicates a gradual accumulation towards their

centre, where they are sufficiently condensed to produce the appearance of a nucleus. These we are told are the most magnificent objects that can be seen in the heavens.

7. *Nebulæ.*—These, it is thought, may be resolved into the three last-mentioned species, only removed to such a distance that they can only be seen by means of the most powerful telescopes.

8. *Stars with burrs, stellar Nebulæ.*—These are thought to be clusters of stars, at great distances, the light of which is gathered so nearly into one point, as to leave but just enough of it visible to produce the appearance of burrs.

9. *Milky nebulosities.*—These phænomena are probably of two different kinds, one of them being deceptions; namely, such as arise from extensive regions of closely connected clustering stars contiguous to each other, like those that compose our milky way: the other, on the contrary, being real, and possibly at no very great distance from us. The milky nebulosity of Orion, discovered by Huygens, is given as an instance of this singular appearance.

10. *Nebulous stars.*—Whether these be the effect of the atmospheres of certain stars remains yet to be determined; and indeed every thing respecting the nature of these appearances is still involved in much doubt and obscurity.

11. *Planetary Nebulæ*; and 12. *Planetary Nebulæ with centres.*—These also, though objects manifestly distinct from the former ones, are as yet so imperfectly known, as to baffle all reasoning concerning their nature and habits; and Dr. Herschel contents himself for the present with merely inserting the few he has observed in his catalogue.

Here follows the copious catalogue of Nebulæ, &c., which being a continuation of two preceding papers of the like nature, and arranged in the same manner, requires no further explanation.

The Bakerian *Lecture. Observations on the Quantity of horizontal Refraction; with a Method of measuring the Dip at Sea. By* William Hyde Wollaston, *M.D. F.R.S.* Read November 11, 1802. [*Phil. Trans.* 1803, *p.* 1.]

In a communication on this subject, published in the volume of the Philosophical Transactions for the year 1800, Dr. Wollaston accounted for various singular phænomena of horizontal refraction by certain gradual changes in the density of the refracting medium. Having since perused what M. Monge has published in the *Mémoires sur l'Egypte*, concerning the appearance known to the French by the name of *Mirage*, where it is ascribed to permanent rarefied strata of air near the surface of the earth; our author, having reconsidered the subject, and finding that the facts related by the French philosopher accord entirely with his own theory, declares here that he still adheres to his former opinion, and assigns his reasons for not departing from it.

The chief of these reasons is, that the definite reflecting surface,

which M. Monge supposes to take place between two strata of air of different density, is by no means consistent with that continued ascent of rarefied air which he himself admits; and that the explanation founded on this hypothesis will not apply to other cases, which may all be satisfactorily accounted for, upon the supposition of a gradual change of density, and successive curvature of the rays of light by refraction.

The subject being of far greater importance than may at first sight appear, since the variations in the dip of the apparent horizon, on which all observations of altitude at sea necessarily depend, must be influenced by this variable refraction, our author has been vigilant in availing himself of every incident that might serve to throw some light on the subject: among these, the first that occurred was an appearance he saw on the river Thames; when being seated in a boat, with his eye about half a yard above the surface of the water, he perceived the oars of barges at some distance, bending inwards, the point of curvature or angle taking place at a small height above the sensible horizon.

He now recollected that the warmth of the summer having been very considerable, the temperature the water had acquired, and still retained when the atmosphere had become cooler, must occasion a rarefaction of the stratum of air above its surface greater than those at higher elevations.

This led him to a series of further observations, which he has collected in a table, from which we learn that, taking in likewise the hygrometrical changes in the atmosphere, the depression of the horizon is greater the higher the temperature of the water is above that of the air; but that this depression is materially diminished by the increasing dryness of the air.

That these refractions (which in the above-mentioned observations were by no means at all times consistent,) must be affected by the vicinity of land influencing the temperature of the air, will be easily admitted; and hence the observations at sea may, it is thought, afford some more accurate conclusions, though the quantity of depression may not be so great. Thus much however is evident, that the error in nautical observations, arising from a supposition that the horizon is invariably according to the height of the observer, stands greatly in need of correction.

How to apply this correction is the object of the close of this paper. This consists in measuring, by a back observation, the whole vertical angle between any two opposite points of the horizon, either before or after taking an altitude, and calculating half the excess of this angle above 180°, which will of course be the dip required.

A few cautions are lastly given for correcting some inaccuracies in the instruments, especially the index error in the back observations, which it is owned had been some years since suggested by Mr. Ludlam.

A chemical Analysis of some Calamines. By James Smithson, *Esq. F.R.S.* Read November 18, 1802. [*Phil. Trans.* 1803, *p.* 12.]

The uncertainty that has till now prevailed concerning the nature and composition of the ores of zinc called Calamine, has induced our author to enter upon the investigation now before us. In the first part of the paper, we find the analysis of four kinds of calamines; the first from Bleyberg in Carinthia, the second from the Mendip hills in Somersetshire, the third from Derbyshire, and the fourth an electrical calamine from Regbania in Hungary. Referring to the paper for the detail of the four processes there circumstantially described, we must content ourselves with reciting here the results deduced from each of them.

1000 parts of the Bleyberg ore were found to consist of 714 calx of zinc, 135 carbonic acid, and 151 water. Some carbonate of lime and lead were likewise found in it; but these appeared to be mere accidental admixtures, and in too small quantities to deserve notice.

1000 parts of the Mendip ore consisted of 648 parts of calx of zinc, and 352 of carbonic acid, and yielded no water.

In the Derbyshire ore were found 652 of calx of zinc, and 348 of carbonic acid.

And in the Hungarian ore, 683 of calx of zinc, 250 of quartz, 44 water: and here there moreover appeared a loss of 23, owing, no doubt, to some defect in the manipulation. The water was by no means considered as an essential part of this ore; and hence the proportions of the two other ingredients were as 739 to 261.

In a second part of the paper, the author communicates some observations to which he was led by the uncertainty that still prevails in our chemical researches, and the want of uniformity in the results of the multitude of experiments that are daily made, which appear to him to clash essentially with the simplicity of nature. When we consider, he says, the simplicity found in all those parts of nature which are sufficiently known to come within the reach of our observation, it appears improbable that the constituent parts of bodies, which we consider as endowed with reciprocal affinities, should be so loosely united as is often indicated by the most accurate analysis. Hence he is led to conjecture, that in all chemical combinations, those ingredients which are really essential to the compound are but few in number; that they are by nature certain aliquot parts of the whole compound; and that as the aliquot may be expressed by fractions, the denomination of these fractions will always be a small quantity, perhaps never exceeding the number 5.

The author applies this theory to the above-mentioned experiments on calamine; and finding that, with a trifling correction, the results coincide with this theory, he entertains sanguine hopes that future investigations will finally establish it. If so, he thinks that the discovery will introduce in chemistry a rigorous accuracy, of which it has not hitherto been thought susceptible; that it will enable the chemist, like the geometrician, to rectify by calculation the unavoid-

able errors of his manual operations, and authorize him to eliminate from the essential elements of a compound those products of an analysis whose quantity cannot be reduced to any admissible proportion, and may therefore be considered as extraneous.

The author, at the close of his paper, controverts the opinion of those who think that crystallization requires a previous state of solution in the matter crystallized; and contends, that as long as any quantity of fluid is present in a solution, no crystallization can possibly take place.

Experiments on the Quantity of Gases absorbed by Water, at different Temperatures, and under different Pressures. By Mr. William Henry. *Communicated by the Right Hon. Sir* Joseph Banks, *K.B. P.R.S.* Read December 23, 1802. [*Phil. Trans.* 1803, *p.* 29.]

After a short recapitulation of what has of late been done by Mr. Cavendish, Dr. Priestley, Dr. Nooth, and others, respecting the impregnation of water with different gases, our author observes, that the circumstance of the different degrees of temperature and pressure had not been as yet sufficiently attended to. Dr. Priestley, indeed, had long since remarked, that, in an exhausted receiver, Pyrmont water will actually boil at a common temperature, by the copious discharge of its air; and that hence it is very probable, that by means of a condensing engine, water might be much more highly impregnated with the virtues of the Pyrmont spring: but this conjecture remained as yet to be proved by experiments; and this is the task our author has undertaken in the present paper.

This paper consists of two sections; the first treating of the quantities of gases absorbed by water under the usual pressure of the atmosphere; and the second, of the influence of pressure in promoting the absorption of gases. The apparatus contrived for these experiments may be described as a siphon, of which one side, or leg, is a glass vessel of comparatively a considerable diameter, and the other a long glass tube of about a quarter of an inch bore; the junction of these two parts at the bottom being a short pipe of India rubber, well secured by proper integuments of leather, thus forming a joint, which admits of the vessel being briskly agitated. This vessel has a stop-cock both at top and bottom, in order to insert and emit fluids and gases; and both the vessel and tube are accurately graduated. It may now be understood, that a known quantity of water and of a certain gas being put in the vessel, and the tube being filled to a certain extent with mercury, the absorption of the gas will be accurately measured by the column of mercury in the tube. Those who are particularly interested in this inquiry will find in the paper various precautions and additional contrivances, all tending to insure the success and accuracy of the investigation.

The first experiments were made on the absorption of carbonic acid gas by water: and here a singular disagreement was observed in the first trials made under exactly the same circumstances. It

soon occurred that this might be owing to the variable amount of the residua of the gas, after the absorption; and this was actually confirmed by the observation, that, of a greater quantity of gas, more would be absorbed than of a smaller, though both quantities were sufficient for saturation of equal quantities of water. This was found to be owing to the quantity of common air, which will ever be extricated from the water, though it be ever so pure, and which will form a greater proportion of the smaller than of the greater dose of the residuary gas.

A table of nine experiments is next given, in which are entered the temperature, the quantities of water and gas, the quantities of gas absorbed, the residua, and the quantities absorbed by 100 inches of water. The two extreme results are, that, at the temperature of 55°, 13 measures of water, exposed to 32 measures of gas, absorbed 14 measures, leaving a residuum of 18 measures; so that the absorption of 100 measures of water would be 108 measures of gas. In the temperature of 110°, 20 measures of water, exposed to 20 measures of gas, absorbed 9 and left 11; so that 45 in 100 was the total of the absorption.

A series of experiments on other less absorbable gases have afforded for one temperature, viz. 60°, and in 100 cubic inches of water, the following results:—nitrous gas 5 inches, oxygenous gas 2·63, phosphorated hydrogen gas 2·14, azotic gas 1·20, and hydrogen gas 1·08. Some experiments are next described on the quantity of atmospherical air that may be extricated from water; the general result of which is, that 100 cubic inches of common spring water will yield 4·76 of gas; which, being analysed, was found to consist of 3·38 carbonic acid, and 1·38 atmospherical air.

The object of the second section being to ascertain the ratio between the addition of pressure and the increased absorption of gases by water, Mr. Henry made some alteration in his apparatus, which consisted chiefly in lengthening the tube, so that, by the addition of mercury, any required addition of pressure might be obtained on the water and gases.

The results of a series of at least fifty experiments on a variety of gases were, that under equal circumstances of temperature, water takes up, in all cases, the same volume of condensed gas as of gas under ordinary pressure; but that as the spaces occupied by every gas are inversely as the compressing force, it follows that water takes up of gas, condensed by one, two, or three additional atmospheres, a quantity which, ordinarily compressed, would be equal to twice, thrice, &c. the volume absorbed under the common pressure of the atmosphere.

Experiments and Observations on the various Alloys, on the specific Gravity, and on the comparative Wear of Gold. Being the Substance of a Report made to the Right Honourable the Lords of the Committee of Privy Council, appointed to take into Consideration the State of the Coins of this Kingdom, and the present Establishment and Constitution of His Majesty's Mint. By Charles Hatchett, *Esq. F.R.S.* Read January 13, 1803. [*Phil. Trans.* 1803, *p.* 43.]

From the introduction to this paper we learn, that in the year 1798, His Majesty was pleased to appoint a committee of members of his Privy Council, to take into consideration the state of the coins of the kingdom; and that this committee, having remarked the considerable loss which the gold coin in particular had sustained by wear within certain periods, had applied to Mr. Cavendish and Mr. Hatchett for their opinion what were the causes of this diminution, and what remedy might be applied to the defects by which it is occasioned. The mode of carrying on this investigation having been agreed upon by these two gentlemen, it fell to Mr. Hatchett's lot to perform the preconcerted experiments, and to draw up the account of them. Of this account, as it was too voluminous, and consisted of too many tables to be read in public, Mr. Hatchett has been pleased to communicate to the Society the Abstract, the reading of which took up the whole of this and the preceding meeting. On a general contemplation of the subject, it soon occurred that the inquiry was to be directed to two principal points;—1st, which of the two sorts of gold, whether that which is very ductile, or that which is as hard as is compatible with the process of coining, suffers the greatest loss under the general circumstances of friction;—and 2dly, whether coins with flat, smooth, and broad surfaces, wear less or more than coins which have certain protuberant parts raised above the ground or general level of the pieces. With a view of arriving at some certain data respecting these questions, three objects were principally kept in view, which gave rise to the three sections that compose the body of the paper. The first of these comprehends the chemical experiments, those which relate to the effects produced upon gold by the addition of different metals in certain relative proportions;—the second includes those experiments which relate to the different degrees of density observed in gold when differently alloyed;—and the third consists of those experiments which may be called mechanical, and which were expressly intended to ascertain the comparative wear of different kinds of gold by various modes of friction.

In the numerous set of experiments which are described in the first section, the effects of every metal and semi-metal upon the colour and ductility of gold were ascertained with all possible care and precision. All the semi-metals were found to affect the quality of gold too essentially, though in different degrees, to be ever used as alloys. And among the metals, lead in very small proportions was likewise found to render gold so completely brittle, as to be absolutely unfit for coinage. Tin was not near so pernicious; and iron, though it

turned gold much paler, yet did not materially affect its ductility. With respect to platina, one-twelfth of this metal, alloyed with gold, turned the latter metal to a colour similar to that of tarnished silver, but did not essentially diminish its ductility. Hence it is inferred, that a mixture of platina with gold, with a view to the adulteration of coin, need not be so much apprehended as was once the case, since the remarkable change of colour is a sufficient criterion to detect the fraud. The ultimate results of the experiments on copper and silver are, that these, either jointly or separately, are the only metals fit for alloys to reduce fine gold to the standard; care only must be taken that they, especially the copper, be of the purest sort; for which purpose, the fine granulated Swedish copper is recommended as the most proper. A mixture of the two metals ought to have the preference, as the colour of the gold is least affected by it.

2. In examining, in the second section, the specific gravity of gold made standard by different metals, single or mixed, it was found that several variations take place from causes independent of any defects in the hydrostatical operations. These are imputed to occasional imperfections in the interior texture of the mass during the processes of melting and casting; to a difference of density in parts of even the same mass; to the nature and position of the mould in which the metal is cast,—a long mould in a vertical position always producing a bar of metal more dense at the bottom than towards the top; to peculiar effects which certain metals produce when employed as alloys, and which are often very different from the results of calculation; and, lastly, to the effect of friction, which, as it is well known to generate heat, cannot, by the expansion it occasions, but affect the specific gravity of the metal. It hence follows, that as the specific gravity of metals is liable to be influenced by such a numerous variety of causes, it is almost in vain to expect absolute precision in the results of such experiments, and that a near approximation is all that can be demanded.

From the experiments made upon separate and entire ingots of gold, reduced to standard by silver and copper, separately and conjointly, it was proved that their specific gravities were as follows:—gold made standard by silver, 17·927; gold made standard by equal parts of silver and copper, 17·344; and gold made standard by copper, 17·157. Hence it appears that the specific gravity of our gold coin, which is generally alloyed by a mixture of the two metals, must be found somewhere between the two extremes just now mentioned; or, making allowances for small variations, arising from accidental causes, between 18 and 17.

3. In the third section, which treats of the comparative wear of gold when variously alloyed, we find, in the first place, an account of three modes or contrivances for ascertaining the quantity of abrasion by friction, according to the different circumstances of alloy and figure in the coins. In the first, two sets of coins were fastened, each in a frame, one of which was made to move backwards and forwards over the other with certain determined degrees of velocity

and pressure. In the second, 200 pieces of gold differently alloyed were inclosed within a wooden box, which was kept constantly turning round, until, by the continued rubbing and striking of the pieces against each other, and against the sides of the box, they were found to be perceptibly diminished. And in the third mode, the pieces to be examined were pressed against the rim of a flat horizontal wheel, by means of equal weights, so that by turning the wheel round, they all suffered an equal degree of friction. The part of the wheel against which the pieces rubbed was sprinkled or coated with some kind of powder, which was varied in the different experiments.

The general results of the many experiments made with this apparatus were, 1. That when equal friction, assisted by a moderate pressure, takes place between pieces of coin which are in each series of a similar quality, then, abrasion is most commonly produced in an inverse ratio to the degree of ductility;—2. That the contrary effect happens when pieces of different qualities rub against each other, the more ductile metal being then worn by that which is harder;—and 3. That earthy powders and metallic filings produce similar effects, and tend to wear the different kinds of gold in proportion to their respective degrees of ductility.

The practical inferences to be deduced from these results are, that pure gold, being extremely ductile, is not the most proper to be formed into coin; that gold, on the other hand, brought by its alloy to the greatest degree of hardness that will bear the manipulation of coining, will be so destructive to the instruments in the Mint, as to render the expense occasioned by this detriment much greater than the small saving that would accrue from the greater durability of the metal; and that hence gold of a moderate ductility must be that which is best adapted for coin, which degree of ductility will be found in the standard proportion of one-twelfth of alloy consisting of about equal parts of silver and copper.

Several incidental circumstances are mentioned at the close of this paper, some of which relate to the cause of the changes of colour in gold coins, which are ascribed to certain chemical changes in the alloy near the surface of the piece during the processes of annealing and blanching. We are also informed that the obliteration of the impressions on gold coins is not always attended with a diminution of weight, but that the supposed abrasion of the prominent parts is in fact a depression of those parts into the mass, bringing them to a level with the rest.

Upon the whole, our author concludes that the great loss which the gold coin of this kingdom is stated to have sustained, cannot possibly be attributed to any important defect in the composition or quality of the standard gold; and that all that can be said upon this subject is, that some portion of this loss may have been caused by the rough impression and milled edge now in use, by which each piece of coin acts and is acted upon by the others, in the manner of a file or rasp.

Observations on the chemical Nature of the Humours of the Eye. By Richard Chenevix, *Esq. F.R.S. and M.R.I.A.* Read November 5, 1802. [*Phil. Trans.* 1803, *p.* 195.]

After a brief survey of what former physiologists, such as Bertrandi, Fourcroy, Wintringham, and Chrouet, have done in investigating the chemical history of the humours of the eye, in all which he found so considerable a disagreement, and so much obscurity, as to render a new analysis at least useful if not necessary, Mr. Chenevix proceeds to give us his own experiments. Of these, the first series was made on the eyes of sheep, and the second on the human eye; and they of course relate to the three humours, the aqueous, the crystalline, and the vitreous.

The specific gravity of the aqueous humour of the sheep's eye was found to be 10·090, taking that of water as 10·000; and from the results of various experiments, it appears to be composed of water, albumen, gelatine, and of a muriate the basis of which was found to be soda. The specific gravity of the crystalline was equal to 11·000, and was found to consist of a smaller quantity of water than the other humours, but of much larger proportion of albumen and gelatine; and no essential difference could be perceived between the vitreous humour and the aqueous, their specific gravities, as well as their several chemical properties, being the same.

From the examination of the humours of the human eye, Mr. Chenevix convinced himself that they are scarcely in any respect different from those of the sheep's eye. The aqueous and vitreous humours were found to contain water, albumen, gelatine, and muriate of soda; the latter ingredient alone being wanting in the crystalline. The specific gravities of the aqueous and vitreous humours were 10·053, and of the crystalline 10·790.

The specific gravity of the crystalline, compared with that of the aqueous and vitreous humours, being much greater in the human eye than in that of the sheep, our author bestows some pages on an inquiry concerning this phænomenon; the result of which is, that as the globe of the human eye is smaller than that of the sheep, and hence the distance from the cornea to the retina much shorter, nature, in order to preserve the achromatic effect of the eye, rendered the human crystalline proportionably more dense than in other animals. This illustration is confirmed by the examination of the eye of an ox, where the difference between the specific gravities of the humours was as 10·088 to 10·765.

In examining the eyes of birds, it was found, that, different from those of quadrupeds, the cornea, or the anterior part of the eye, is a portion of a larger sphere than the sclerotica, or posterior part of the ball. It is hence obvious that, in order to produce a proper refraction, it is necessary that the densities of the humours be essentially different. Accordingly, it was found that the specific gravity of the vitreous humour was 11·210; while that of the crystalline was no more than 10·392. Whence it appears, that the densities are here

actually inverted, in order to suit this structure of the eye. The humours of the eyes of birds are chemically of the same nature as those of quadrupeds.

It is also observed, that the crystalline in all animals is not throughout of the same density; the result of some experiments made on this subject being that its density increases from the circumference to the centre, as the square roots of the quantities pared away from the external part.

Lastly, it is suggested, that since we know that albumen can be coagulated by various methods, it is not unlikely that this may happen likewise in the human eye, and be the cause of disorder known by the name of Cataract. An attention to this complaint, especially in gouty persons, is strongly recommended; as some important conclusions, it is thought, may be drawn as to the influence of phosphoric acid in causing that disorder, by the common effect of acids in coagulating albumen.

An Account of some Stones said to have fallen on the Earth in France; and of a Lump of native Iron, said to have fallen in India. By the Right Hon. Charles Greville, *F.R.S.* Read January 27, 1803. [*Phil. Trans.* 1803, *p.* 200.]

Mr. Greville, conceiving that the experiments and observations made by Mr. Howard on certain metalline substances said to have fallen on the earth, and the accurate descriptions which Count de Bournon has given of these substances, have established the fact that a number of stones, asserted to have fallen under similar circumstances, have precisely the same character, is here pleased to communicate to the Society three more instances of such singular productions of nature, which have of late been noticed in France.

The first is a specimen broken from a stone of about 15 inches diameter, preserved in the Museum of Bourdeaux, and which is said to have fallen near Roqueford, in the Landes, on the 20th of August, 1789, during the explosion of a meteor. It broke through the roof of a cottage, and killed a herdsman and some cattle.

The second is part of a stone preserved in the collection of Mons. St. Amand, which was one of the numbers that fell in the year 1790, in three different parishes in Armagnac, some of which weighed no less than 25 pounds. The fact of this shower of stones was at the time verified by the Mayor of Armile, and is published in the *Journal des Sciences Utiles de Montpellier* for that year. For the third specimen Mr. Greville is indebted to the Marquis de Drée. It is a fragment broken from a stone of 22 pounds weight, which fell near Villefranche, in Burgundy, the 12th of March, 1798. This, like the former ones, was accompanied by a meteor; and all three have precisely the same character, texture, and appearance.

We are indebted to Mr. Greville for a new evidence, and he says, the only one he has yet met with, that seems to ascertain the origin of native iron, which, from analysis, had been suspected to have a

common origin with the stones fallen on the earth. This he obtained from Colonel Kirkpatrick, and it consists of a translation from the Persian, made by the Colonel, of a passage in the Memoirs of the Emperor Jehangire, written by himself. The substance of this extract is as follows: In the first year of this Emperor's reign (A. 1030 of the Hegira) there arose one morning in a village, about 100 miles East of Lahore, such a tremendous noise as had near deprived the inhabitants of their sense of hearing. During this noise, a luminous body was observed to fall from above on the earth, suggesting to the beholders the idea that the firmament was pouring fire. In a short time the noise having subsided, and the inhabitants having recovered from their alarm, a messenger was dispatched by them to the Aumil, or fiscal superintendant of the district, to apprise him of the event. This magistrate immediately repaired to the spot, and there perceived that the earth, to an extent of about ten or twelve yards in diameter, was burnt to such a degree that not a blade of grass nor the least trace of verdure remained; nor had the heat, which had been communicated to it, as yet subsided.

The Aumil hereupon caused the aforesaid space of ground to be dug up. The deeper they went the greater was the heat found to be. At length a lump of iron made its appearance, the heat of which was so great that one might have supposed it to have been taken from a furnace. After some time it became cold, when the Aumil conveyed it to his own habitation, from whence he dispatched it to court.

Here (the Emperor says) I had it weighed in my presence, and found its weight to fall little short of 80 ounces. I committed it to a skilful artist, with orders to make it into a sabre, a knife, and a dagger; but the workman soon reported to me that the substance would not bear the hammer, but shivered into pieces when struck. Upon this I ordered it to be mixed with other iron. Accordingly three parts of this *iron of lightning*, as we called it, were mixed with one part of common iron; and from this mixture were made two sabres, one knife, and one dagger. By the addition of the common iron, the new substance acquired a fine temper, the blades fabricated from it proving as elastic as the most perfect that can be made in our country. I had them tried in my presence, and found that they cut admirably. One of the sabres I called *Katai*, or the Cutter, and the other *Busk-serisht*, or the Lightning-natured.

In a tetrastich presented to the Emperor on this occasion, it is asserted that in his time fell raw iron from lightning. Colonel Kirkpatrick certifies the genuineness of the manuscript, and the fidelity of the translation; and Mr. Greville adds, that he considers this as an authentic fact, the Emperor Jehangire not being a prince on whom his courtiers would idly venture to impose, or to whom an Aumil of a district would have dared to produce a substance pretending it to be iron, which on trial should be found to differ from manufactured iron.

Observations on the Structure of the Tongue; illustrated by Cases in which a Portion of that Organ has been removed by Ligature. By Everard Home, *Esq. F.R.S.* Read February 3, 1803. [*Phil. Trans.* 1803, *p.* 205.]

These observations will be allowed to have a considerable degree of importance, when we find that they ultimately lead to a safe and effectual method of removing a portion of the tongue, when that organ has assumed a diseased action or morbid excrescences of a cancerous nature, to which this, as well as many other glandular structures, are known to be liable. In a physiological view they will likewise be found to merit particular attention, as they tend to prove that the internal structure of the tongue is not of that delicate and sensible nature which, from its being the organ of taste, we should be led to imagine.

The first case here mentioned, and from which various inferences are derived which lead to a new mode of treating the disorders of the tongue, was that of a gentleman whose tongue had been accidentally bit near the tip, and had hence become completely insensible, insomuch that every article of nourishment he took was equally insipid, and that the tip felt like a bit of wood in his mouth. No degree of inflammation, however, or spasmodic tendency having accompanied these symptoms, Mr. Home inferred that the nerves supplying this, and perhaps the other organs of sense, are not so liable to irritation as those which belong to other parts of the body.

Encouraged by this observation, he in three instances performed a new operation upon the tongue, which consisted in removing a portion of that organ by means of ligatures, and with that portion certain fungous excrescences which might have been productive of fatal consequences. The first patient was a boy eight years of age, who had been born with a small excrescence on the right side of the anterior part of the tongue. It had been removed no less than eleven times by ligatures round its base, caustics and amputation, but always with considerable and dangerous hæmorrhages; and after all without success, the fungus always reappearing soon after the operations. Mr. Home at length resolved to take out the portion of the tongue upon which the fungus grew. This was effected by passing a crooked needle, armed with a double ligature, through the substance of the tongue, somewhat within the excrescence: the needle was drawn out below, leaving the ligatures, one of which was tied very tight before the excrescence, and the other equally so behind it, so that a segment of the tongue was confined between these two ligatures, in which the circulation was completely stopped. On the fifth day after the operation this portion of the tongue came away with the ligatures, leaving a sloughy surface, which likewise separated on the fifteenth day. The excavation a few days after this became completely cicatrized, leaving only a small fissure on that side of the tongue.

The two other operations were performed on persons above forty

years of age. In these cases the excrescence was a tumour no larger than a pea, but the appearance of which threatened the formation of a cancer. Both these were successfully removed by the ligatures above described; and, except a small fissure, no kind of deformity was left upon the tongue.

The principal inference derived from these operations is, that the internal structure of the tongue is less irritable than almost any other organized part of the body; and that therefore the peculiar substance which is interposed between the fasciculi of its muscular fibres is not in any respect connected with the nerves which pass through its substance to the organ of taste, but is merely a soft medium, which is intended to facilitate the action of the organ in its different parts. It also appears from these observations, that the nerves of the tongue may be more easily compressed and deprived of their power of communicating sensation than nerves in general; and that an injury inflicted on them is not productive of any diseased action in the trunk of the injured nerve.

Lastly, the advantages to be derived in the practice of surgery from the success of these operations, are briefly stated; and indeed the removing with safety the whole part of a tongue which may have taken on a disposition to be cancerous, will easily be allowed to be a most material improvement in that important branch of the medical profession.

Observations of the Transit of Mercury over the Disc of the Sun; to which is added, an Investigation of the Causes which often prevent the proper Action of Mirrors. By William Herschel, *LL.D. F.R.S.* Read February 10, 1803. [*Phil. Trans.* 1803, *p.* 214.]

Concerning the transit of Mercury, as the times at which the observations were made were not the chief object of the investigation, the detail here given is only to be considered as denoting the order of their succession. When the planet was first seen on the disc of the sun on the 9th of November last at about 40′ after seven in the morning, it was easily distinguished from the openings in the luminous clouds generally called spots, its perfect roundness being sufficient to point it out, had its place not been previously known. As the morning advanced, its termination became by degrees still more accurately defined; and the corrugations of the luminous surface of the sun were visible up to the very edge of the planet. Near the egress, when the sun and planet were nearly in the meridian, particular attention was paid to the appearance that was thought to indicate an atmosphere round Mercury; but nothing of the kind could be perceived, its periphery remaining sharp and well defined to the very last. It was also observed that the appearance of the planet, during the whole transit, never deviated in the least from the spherical form; whence it is inferred, that unless its polar axis should have happened to be situated in a line drawn from the eye to the sun, the planet cannot be materially flattened at its poles.

Concerning the second part of this paper, namely, the causes which often affect mirrors so as to prevent their showing objects distinctly, though it be well known to astronomers that telescopes will act very differently at different times, yet no particular inquiry had yet been made respecting the cause of this imperfection. The experience our author has acquired during his long series of observations, in which he never lost sight of this circumstance, has enabled him to combine a set of facts, from which he thinks himself authorized to deduce inferences which will be found to throw a considerable light upon the subject.

These observations are here described at length, and arranged under different heads, chiefly according to the state of the atmosphere at the time they were made. Their results will in some measure point out the nature of them. They seem to establish, as a general principle, that in order to see distinctly with "telescopes, it is required that the temperature of the atmosphere and mirror should be uniform, and that the air be impregnated with moisture." Hence it appears that a frost after mild weather, or a thaw after frost, will sensibly derange the performance of our mirrors, till either the frost or the mild weather are sufficiently settled that the temperature of the mirror, and indeed of the whole telescope, may accommodate itself to that of the air. That when a frost, though very severe, becomes settled, the mirror will soon accommodate itself to the temperature, and the telescope will be found to act well. That no telescope brought into a cold atmosphere out of a warm room, can for a time be expected to act properly; and that no delicate observations, with high magnifying powers, can well be made when looking through a door, window, or slit in the roof of an observatory. It equally appears that windy weather in general, which must occasion a mixture of airs of different temperatures, cannot be favourable to distinct vision: and that the auroræ boreales, when they induce, as they often do, a considerable change in the temperature of the different regions of air, are likewise detrimental as to distinctness.

Sometimes the weather may be perfectly serene, and yet the telescopes will act imperfectly. This may be owing to the dryness occasioned by easterly winds, or by a change of temperature arising from an agitation of the upper regions of the atmosphere, or perhaps by both these causes combined together.

Dry air, it seems, is by no means proper for vision; and hence dampness, haziness, and fogs, to a certain degree, will generally be found favourable to distinctness: damp situations, therefore, and the neighbourhood of lakes or rivers, need not be objected to in choosing a spot for an observ tory. As the warm exhalations of the roof of a house in a cold night must disturb the uniformity of the temperature of a certain contiguous portion of air, it is to be expected that the appearance of stars seen over a house, and at no considerable distance from it, will be affected by that emanation.

Lastly, one of the most essential causes of the want of uniformity in the performance of telescopes must, it seems, be ascribed to the

effects of heat and cold upon the figure of the mirrors. This circumstance has called forth a particular investigation, in which heated bodies were approached at different distances, both before and behind mirrors, either of glass or metal; and it was remarkable how their focal lengths were immediately affected by it. Hence it may reasonably be inferred, that the rays of the sun on a mirror will produce a similar distortion. That the dilatation occasioned by heat is the cause of this defect, will easily be admitted; but our author does not enter here upon the theory of this influence, nor upon the remedies that may be applied to its detrimental consequences. These points he reserves for a future communication.

An Account of some Experiments and Observations on the constituent Parts of certain astringent Vegetables; and on their Operation in Tanning. By Humphry Davy, *Esq. Professor of Chemistry in the Royal Institution. Communicated by the Right Hon. Sir* Joseph Banks, *Bart. K.B. P.R.S.* Read February 24, 1803. [*Phil. Trans.* 1803, *p.* 233.]

The importance of the subject handled in this paper, which, as it particularly relates to the process of tanning leather, will be allowed to be of sufficient magnitude, has of late excited the attention of several able philosophers, among whom Mr. Seguin was the first who ascertained the peculiar vegetable matter which is essential to this process, and which is possessed of the characteristic property of precipitating gelatine from its solutions. Mr. Proust has since investigated many other properties of this substance; but neither these, nor any other chemists, have as yet carried their investigations so far as to determine the various affinities of tannin, and especially how its action upon animal matters is modified by combination with other substances. This task was reserved for our author, who during the two last years bestowed most of his leisure hours on a course of experiments on this subject; and he here lays before the Society an account of their general results. His chief design was to elucidate the practical part of the process; but in pursuing it he found himself necessarily led to general chemical inquiries concerning the analysis of the different vegetable substances containing tannin, and their peculiar properties.

The paper consists of five parts, the titles of which are as follows: —1. Observations on the analysis of astringent vegetable infusions. 2. Experiments on the infusions of galls. 3. Experiments and observations on the extracts of Catechu, or Terra Japonica. 4. Experiments and observations on the astringent infusions of barks, and other vegetable productions; and 5. General observations.

And first, as to the analysis of astringent vegetable infusions; the substances that have been supposed to exist most generally in them are tannin, gallic acid, and extractive matter. The presence of tannin in an infusion is denoted by the precipitate it forms with gelatine, such as glue or isinglass; but the process requires many and

very delicate precautions: previous to the experiments the infusions are materially affected by exposure to the atmosphere; the tanning principle in different vegetables demands for its saturation different proportions of gelatine; and the quantity of the precipitate obtained by filtration is not always proportionate to the quantity of tannin and gelatine in the solution, but is materially influenced by the degree of their concentration. Hence it follows that the solutions of gelatine, for the purposes of analysis, should be employed in as high a state of saturation as is compatible with their perfect fluidity. They should be used only when quite fresh; and as their relative effects were found to be influenced by their temperature, it was found expedient to bring them, and the infusions on which they were designed to act, as nearly as possible, to a common degree of heat: great care must also be taken to prevent any excess of gelatine. Duly attending to all these precautions, the general result is, that in any given case, when the quantity of gelatine in the solution employed upon an astringent infusion is compared with the quantity of the precipitate obtained, the difference between them may be considered as the proportion of tannin contained in the infusion.

The tannin being thus separated, it remained to ascertain the proportion of the two other ingredients in the infusion, viz. the gallic acid, and the extractive matter. The first step here was slow evaporation, by which the latter substance is in part rendered insoluble, so as to subside at the bottom of the vessel. A proportionate quantity of alcohol being next poured upon the fluid thus reduced to a thick consistency, both the gallic acid and the soluble extractive matter, if there be any remaining in the infusion, will be dissolved. The great difficulty now was to separate the gallic acid and the extractive matter. Ether and alumine were tried without the desired effect, the affinities of these two agents with those substances not being sufficiently distinct to produce the separation. Some judgement, however, may be formed of their relative proportions, by means of the salts of alumine and the oxygenated salts of iron. Muriate of alumine precipitates much of the extractive matter from solutions, without acting materially upon gallic acid; and after this precipitation, some idea may be formed concerning the quantity of the gallic acid, by the colour it gives with the oxygenated sulphate of iron.

2. *Concerning the Infusions of Galls.*—The strongest solution of gall-nuts was obtained by repeatedly pouring distilled water upon the best Aleppo galls, broken into small pieces; it was of the specific gravity 1·068. 400 grains of this solution produced by evaporation 53 grains of solid matter, which, as well as could be estimated by the methods of analysis described in the preceding section, consisted of about $\frac{1}{10}$ths of gallic acid, united to a minute portion of extractive matter. 100 grains, moreover, of this solid matter left, after incineration, nearly $4\frac{1}{4}$ grains of ashes, which were a mixture of lime with carbonate of lime, and a small portion of fixed alkali.

Here follows a long series of experiments on the infusion of gall-nuts, in which it was exposed to, or combined with, all manner of

chemical agents, viz. the sulphuric, muriatic, and nitric acids, potash, soda, ammonia, and a variety of alkaline earths, magnesia, alumine, different solutions of neutral salts, metallic oxides and solutions, and other substances, the list of which is too long to be here inserted. Many of the results may be considered as insulated facts; but those who attend to inquiries of this nature, will find most of them connected with useful conclusions, tending to elucidate this obscure, but no doubt very interesting part of chemistry. The ultimate result of those experiments, as to the constituent parts of gall-nuts, is, that 500 grains of good Aleppo nuts gave by lixiviation with pure water, till all the soluble parts were taken up, 185 grains of solid matter, and that this matter, examined by analysis, consisted of 130 grains of tannin; 35 grains of gallic acid, with a little extractive matter; 12 grains of mucilage, and matter rendered insoluble by evaporation, and the remaining 8 grains of calcareous earth and saline matter.

3. *On the Extracts of Catechu, or Terra Japonica.*—This extract is said to be obtained from the wood of a species of the Mimosa, which is found abundantly in India, by decoction and subsequent evaporation. There are two kinds, the one sent from Bombay, and the other from Bengal. They somewhat differ from each other in their external appearance, but very little, it seems, in their chemical composition. The tastes of both are sensibly astringent; and neither of them deliquesces, or is apparently changed by exposure to air.

Our President was the first who, noticing the more obvious qualities of this substance, suspected that it contained the tanning principle; and being possessed of a sufficient quantity, he was pleased to supply Mr. Davy with all he wanted for the purpose of a chemical examination. The first experiments showed that its solution copiously precipitated gelatine, and that it speedily tanned skin. And hence he was encouraged to undertake the particular investigation of its properties, the account of which is the subject of the present section.

His mode of proceeding, of course, could not differ materially from that which he adopted in the analysis of the gall-nuts. And indeed most of the same, and some additional chemical agents, have been put to the test. The ultimate analysis has been attended with some difficulty, different specimens of this substance, though to all appearance ever so pure, differing materially among themselves; the natives, for the sake of profit, being apt to adulterate what they sell, either with sand, earthy substances, or other extraneous matter.

Mr. Davy, in order to obviate this difficulty, selected a number of specimens, such as he had reason to think the least tainted, and having reduced them into powder, he found the two sorts to consist of the following ingredients:—200 grains of the extract of catechu from Bombay consisted of 109 grains of tannin, 68 grains of a peculiar extractive matter, 13 grains of mucilage, and 10 grains of residual matter, chiefly sand and calcareous earth. The same quantity of the extract from Bengal yielded 97 grains of tannin, 73 grains of peculiar extractive matter, 16 grains of mucilage, and 14 of residual

matter; viz. sand, with a small quantity of calcareous and aluminous earth.

The importance of the object will, we trust, justify our inserting here what our author has ascertained concerning the application of this substance to tanning. Of two pieces of calf-skin, he tells us, which weighed when dry 132 grains each, and which had been prepared for tanning: one was immersed in a large quantity of the infusion of extract of catechu from Bengal, and the other in an equal portion of the infusion of the extract from Bombay. In less than a month they were both found converted into leather. When freed from moisture by long exposure in the sunshine, they were weighed. The first piece had gained about 34 grains, and the second piece 35½ grains. The colour of the leather was much deeper than that tanned with galls, and on the upper surface it was of a reddish brown. It was not acted on by hot or cold water; and its apparent strength was the same as that of similar leather tanned in the usual manner.

4. *On the Infusions of Barks, and other vegetable productions.*—The experiments described in this section were chiefly made on the strongest infusions of the barks of oak, Leicester willow, and Spanish chestnut: each of them were nearly of the specific gravity denoted by 1·05. Their tastes were alike, strongly astringent: 200 grains of each, on being submitted to evaporation, yielded,—the oak bark 17 grains, and the two other barks about 16½ grains of solid matter; and the tannin afforded by these substances were,—the oak bark 14 grains, the willow bark 14½ grains, and the Spanish chestnut 13 grains. These substances also gave by incineration only a very small quantity of ashes, scarcely $\frac{1}{100}$th part of their original weights; and these ashes consisted chiefly of calcareous earth and alkali, the quantity being greatest from the matter produced from the chestnut bark.

These several infusions were acted on by the acids and pure alkalies in a manner very similar to that adopted with the infusion of galls. No gallic acid whatever could be obtained from any of them; and if any be contained in them, it is imagined that it must be in a state of intimate combination with extractive matter. The proportions of the astringent principle in barks vary considerably according to the age and size of the trees from whence they are taken, and probably also according to the different seasons in which they are gathered. In every astringent bark the interior white part (that is the part next to the wood) contains the largest quantity of tannin: the proportion of extractive matter is generally greatest in the middle or coloured part; but the epidermis seldom furnishes either tannin or extractive matter. A few other circumstances are here added, which ought to influence tanners in the choice of their barks.

The other vegetable infusions examined by Mr. Davy were those of the barks of elm and common willow, of sumach, Mirabola nuts, tea, and some other vegetables of known astringent qualities. The results offer no very material differences; but in general the author remarks, that in all substances possessed of an astringent taste, there is great reason to suspect the existence of tannin; that it may be

found in fruits, in which it is modified by sugar and acids; that he has found it in great abundance in the juice of sloes, and that a friend of his had discovered its presence even in port wine. It also appears that it may exist in a state of combination in different substances, in which its presence cannot be made evident by the common means of solutions of gelatine; and that in these cases, in order to detect its existence, it may be necessary to have recourse to the action of diluted acids.

General Observations.—After a few strictures concerning a conjecture of Mr. Proust, that there are different species of the tanning principle possessed of different properties, and different powers of acting upon re-agents, from which our author thinks himself authorized to dissent, he draws the general conclusion,—that in all the different astringent infusions the tanning principle is found possessed of the same general properties and powers of combination. In all instances it is capable of entering into union with the acids, alkalies, and earths; and of forming insoluble compounds with gelatine and with skin. That in the processes of tanning, if the astringent infusion contain extractive and colouring matter, these as well as tannin enter into chemical combination with the skin; but that in no case is there any reason to believe that gallic acid is absorbed in this process. That hence the different qualities of leather made with the same kind of skin, seem to depend very much upon the different quantities of extractive and colouring matter it contains; the leather prepared by means of infusions of galls being generally found harder, and more liable to crack than that obtained from the infusions of bark.

When skins are slowly tanned in weak solutions of the barks, or of extract of catechu, it combines with a considerable proportion of extractive matter, whereby it is rendered perfectly insoluble in water, and yet soft and very strong. The inference, perhaps the most essential, deduced from this inquiry is, that of all the astringent substances as yet examined, the extracts of catechu are those that contain the largest proportion of tannin, half a pound of this extract being found to produce the same effect in tanning as from four to five pounds of common oak bark.

How material this must be in a country where oak timber is not an object of trivial importance, need not be here insisted upon.—The paper closes with a table, in which oak bark being taken as the standard of comparison as to its quantity of tannin, the different astringent substances are arranged in the order of their powers.

Account of some Experiments on the Descent of the Sap in Trees. In a Letter from Thomas Andrew Knight, *Esq. to the Right Hon. Sir* Joseph Banks, *Bart. K.B. P.R.S.* Read April 21, 1803. [*Phil. Trans.* 1803, *p.* 277.]

In a former paper Mr. Knight related some experiments on trees, from which he inferred that their sap, having been absorbed by the

bark of the root, is carried up by the alburnum, or white wood of the root, the trunk, and the branches; that it passes through what he calls the central vessels into the succulent part of the annual shoot, the leaf-stalk, and the leaf; and that it thence returns to the bark through the returning vessels of the leaf-stalk. The principal object of the present paper is to point out the causes of the descent of the sap through the bark, and the consequent formation of wood.

The causes to which he ascribes this descent are: 1. Gravitation; 2. Motion communicated by winds or other agents; and 3. Capillary attraction, and perhaps some peculiar circumstances in the conformation of the vessels themselves, which renders them better calculated to carry fluids in one direction than in another.

Before he proceeds upon the experiments from which he has deduced these conclusions, he premises a few observations on the functions of the leaf, from which all the descending fluids in the tree appear to be derived. He describes an experiment he made on a leaf of a vine, in which its lower surface being placed in contact with a clean piece of plate glass, this glass was soon found to be covered with a strong dew, which had evidently exhaled from the leaf; and at the end of half an hour so much water was found to have been discharged from the leaf, that it ran from the glass when it was held obliquely. The position of the leaf being then inverted, and its upper surface being brought in contact with the glass, not the slightest portion of moisture appeared, although the leaf was for some time exposed to the full influence of the meridian sun. Hence it is inferred, that the vessels intended for perspiration are confined to the under surface of the leaf, and that these, like the cutaneous lymphatics of the animal œconomy, are also capable of absorbing moisture when the plant is in a state to require it; whereas the upper surface seems chiefly formed for absorbing light; and if anything exhale from it, it is probably vital air, or some other permanently elastic fluid.

Reverting now to the principal object of his paper, the author describes an experiment on a shoot of a vine, which he bent downwards nearly in a perpendicular direction. After it had grown some time in this position, and acquired a ligneous texture, he stripped the bark from a part of it, and thus cut off all communication through the bark between the shoot and the parent stem. Former experiments have shown, that had this shoot grown in its erect position, the lip of the bark above the wound would have shown an accumulation of fresh wood and bark; but in this instance the contrary was found to be the case; the lip next to the stem, which by its position was now uppermost, gave evident signs of this accumulation. This is ascribed to the gravitation of the sap, from the curvature of the shoot down to the lip. The result of this experiment seems to point out one of the causes why perpendicular shoots grow with much greater vigour than those which are inclined or horizontal, they having probably a more perfect and rapid circulation.

The effect of motion on the circulation of the sap was deduced

from the following experiment. Several young standard apple-trees were, by means of stakes and bandages, prevented from yielding to the impulse of the wind up to about the middle of their stems, the upper parts of the stems and the branches being left in their free natural state. In the course of one summer it was found that much new wood had accumulated in the parts which were kept in motion by the wind; whereas the lower parts of the stems and roots had increased very little in size. One of these trees was afterwards confined in such a manner that it could only move in one direction, viz. north and south: thus circumstanced, the diameter of the tree from north to south, in that part of the stem which was most exercised by the wind, exceeded that in the opposite direction, in the following autumn, in the proportion of 13 to 11. Several curious inferences may be hence deduced as to the growth of trees in different situations.

In those which are exposed on high grounds, and are kept in almost continual motion, the sap circulates with great rapidity, and will be accumulated chiefly in the roots and lower parts of the trunk; and hence the diameter of the trunk will diminish rapidly as it recedes from the root: the progress of the ascending sap will of course be impeded, and it will thence cause lateral branches to be produced, the forms of which will be similar to that of the trunk; and thus the growth of an insulated tree on a mountain will be, as we always find it, low and sturdy, and well calculated to resist the heavy gales to which, from its situation, it is constantly exposed. Trees, on the other hand, which grow in clumps or sheltered situations, where, for want of motion, the sap is retarded both in its ascent and descent, will acquire a very different habit, and even their wood a different texture, insomuch that a great deal of the timber found in old buildings in and about London, which has always been considered as Spanish chestnut, appear on close examination to be most evidently forest oak. When a tree is wholly deprived of motion, it often becomes unhealthy, and not unfrequently perishes, apparently owing to the stagnation of the descending sap under the rigid confinement of the lifeless external bark. Stripping off this bark has been found singularly beneficial towards the increase both of the trunk and branches.

As to the third cause of the descent of sap, viz. the capillary attraction and peculiar conformation of the vessels, though the alburnum, consisting of such capillary tubes and vessels, appears manifestly to expand and contract under the various changes of temperature and moisture in the atmosphere; and though the motion thus produced must be in some degree communicated to the bark and other contiguous parts, yet combining the results of all his experiments, our author is inclined to consider gravitation as the most extensive and active cause of motion in the descending fluids of trees. An observation which corroborates his assertion is, that if the sap impelled by causes more powerful than gravitation were to pass and return as freely in the horizontal and pendent as in the perpendicular branches, the growth of each would be equally rapid, or nearly so;

and the horizontal branches would hence soon extend so far and become so bulky, as to render it impossible for the trunk to support them. The principal office here ascribed to the horizontal and spreading branches, is to nourish and support the blossoms and fruit, or seed; little or nothing of the sap being here returned to the parent tree, and hence very feeble powers being wanted in the returning system.

Our author had long entertained an opinion that the ascending fluids in the alburnum and central vessels are everywhere the same, and that the leaf-stalk, the tendril of the vine, the fruit-stalk, and the succulent point of the annual shoot, might in some measure be substituted for each other: experiments have proved his conjecture in many instances to be well founded. In several of these experiments, leaves continued to perform their office when grafted on the fruit-stalk, the tendril, and the succulent shoot of the vine; and the leaf-stalk, the tendril, and the fruit-stalk, equally supplied a branch grafted upon them with nourishment.

On examining the manner in which wounds in trees become covered, an additional proof was found, that the medullary processes, namely, the knobs of wood formed at the junction of a grafted bud, with the stock in which it is inserted, are like every other part of the wood generated by the bark. This is mentioned in contradiction to the opinion still entertained, that the hardest, most durable, and most solid part of the wood, is composed of the soft, cellular, and perishable substance of the medulla. Lastly, some observations are stated, which seem to imply that the sap in its descent may undergo some modification which fits it more effectually to produce wood.

A few remarks on the formation of buds in tuberous rooted plants beneath the ground, are added by way of appendix. These, if the above theory be true, must be formed of matter which has descended from the leaves through the bark. An experiment was made on a potatoe plant by intersecting its runners which connect the tubers with the parent plant, and immersing their ends in a decoction of logwood. In about twenty hours it was found that the decoction had indeed passed along the runners in both directions, but that none had entered the vessels of the parent plant. This result was not unexpected to the author, he being well aware that the matter by which the growing tuber is fed must descend from the leaves through the bark, and that bark cannot absorb coloured infusions.

Inquiries concerning the Nature of a metallic Substance lately sold in London, as a new Metal, under the Title of Palladium. By Richard Chenevix, *Esq. F.R.S. and M.R.I.A.* Read May 12, 1803. [*Phil. Trans.* 1803, *p.* 290.]

In April last, a printed notice was circulated concerning a substance to which the name of Palladium, or new silver, was assigned, and of which samples were offered for sale at Mr. Forster's, in Gerrard Street, Soho. A discovery of such importance did not fail to

excite the curiosity of several mineralogists; and among these, Mr. Chenevix, to whom the manner in which this object was presented to the public appeared suspicious, was among the foremost in procuring a sufficient quantity of the substance to enable him to institute a proper analysis by which the fallacy, if there were any, might be detected. The substance, as exposed to sale, had been worked by art: it had been rolled out in flatting-mills, the largest of the lamina being about three inches in length and half an inch broad, and weighing on an average twenty-five grains each. It had much the appearance of platina; but its specific gravity was so low as from 10·972 to 11·482; that of pure platina in the same state being at least 22. The laminæ were flexible, but not very elastic.

After describing these its physical properties, the author proceeds to a circumstantial account of his chemical analysis, and gives an ample detail of the action, on this metal, of caloric; of simple combustible bodies, such as sulphur and charcoal; of other metals, forming various alloys; of alkalies and acids, in the latter of which he found that the true solvent of palladium was nitro-muriatic acid, which attacked it with great violence, and formed a beautiful solution; and lastly, he mentions the appearance and principal properties of the precipitates from its various solutions. From the results of this investigation we learn, that the vender of this substance was not guilty of misrepresentation when he ascribed to it the following properties:—

1. It dissolves in pure spirit of nitre, and makes a dark red solution.

2. Green vitriol throws it down in the state of a regulus from this solution, as it always does gold from aqua regia.

3. This solution, when evaporated, yields a red calx, that dissolves in spirit of salt or other acids.

4. It is thrown down by mercury and by all the metals except gold, platina, and silver.

5. In a common fire the face of it tarnishes a little, and turns blue; but comes bright again, like other noble metals, on being heated to a greater degree.

6. The greatest heat of a blacksmith's fire will hardly melt it.

7. But if touched while hot with a small bit of sulphur, it runs as easily as zinc.

After maturely considering the results of his experiments, and comparing them with the analogous ones made on the various known metals and combinations of metals, our author acknowledges that he does not find to which of them it may be assimilated. The striking resemblance, however, of several of the precipitates of palladium with that of platina, first led to the suspicion, that if the former be a combination, the latter is no doubt one of its principal ingredients.

The stubborn circumstance of the very low specific gravity appeared one of the most difficult to be surmounted; since an alloy of platina, even with tellurium, the lightest of all metals, would not by calculation give so low a standard.

Reflecting, however, upon the various modifications which substances undergo when in union with each other, and on the variations produced in the laws of affinity by the intervention of new bodies, he was induced to try whether, by the affinity of platina with some other metal easily reduced, and the interference of an intermediate agent, a reduction of both metals might not be brought about, although no such effect could be produced upon each metal when separate. Mercury was thought the most likely to succeed, as being the most reducible; and the intermediate agent adopted was green sulphate of iron. A solution of this sulphate was poured into a salt of platina, and also into a salt of mercury; in neither of which any precipitate took place. The two liquors were then united, and a precipitate, exactly resembling that which is formed by green sulphate of iron in palladium, was instantly formed. This precipitate was collected and exposed to a strong heat, and a metallic substance was obtained, not to be anyways distinguished from palladium.

Thus, after having been baffled in his attempts to discover, by analysis, the component parts of this substance, which he could never bring himself to consider as a new metal, a synthetic process at length led him to the discovery, that the whole pretence was an imposition, and that the substance is, in fact, a combination of platina and mercury; in which the latter, while it marks the most characteristic properties of the former, loses the greater number of its own distinctive qualities.

The singular fact, that an alloy of two metals should be produced, the specific gravity of which is little more than one half of what it ought to be by calculation, is, no doubt, worthy of particular attention; and as quicksilver was in this process brought to a fixed state under circumstances never before observed, a notion might be entertained that the great desideratum in alchemy, the fixation of mercury, was by no means a visionary object. This anomaly of the true and the calculated specific gravities of alloys has been attended to with great caution; and we find the results of the inquiry collected in a table, in which are entered the true and the calculated specific gravities of palladium with seven different metals; and the differences are stated, which vary much more considerably than might have been expected, both in excess and defect, the number representing this difference in the combination with platina being +2·100, and with tin −1·165.

Those who cultivate chemistry with any degree of ardour, will be gratified to see in this paper the pains taken by the author, and the various modes he has devised, to produce this compound metal in its most perfect state of combination. Among various other results, it appears that the specific gravities of the alloys vary according to the proportions of the two ingredients in the following manner:—

	Platina	Mercury	Spec. grav.
	61	39	11·736
	70	30	13·249
	81	19	15·141

Should this alloy ever be found useful in the arts, or for œconomi-

cal purposes, the author owns that other methods of forming it, besides those here suggested, might be contrived: but the general problem, he says, is to combine, in the most intimate manner possible, the greatest quantity of mercury with a given quantity of platina. And he adds, that the principal difficulty in resolving the problem will be to unite such a portion of mercury that the specific gravity of the compound may not exceed 12; and that it may be soluble in nitric acid.

In the course of this inquiry, many instances have occurred which show how much we have yet to learn concerning the nature of mercury and platina. Of the former, we know that it is perpetually varying; and that certain solutions of it will frequently change their state in a few hours: and as to the latter, we are still more in the dark concerning its principal properties. A considerable part of the present paper is taken up in describing some experiments respecting these metals, from which we learn that platina will combine with oxygen, and form a true oxide; that the affinities of platina differ much from what has generally been stated in the tables; and that, in general, the whole doctrine of chemical affinities still offers a field for much investigation. The affinities of metals, which are here experimentally demonstrated in several instances, will, no doubt, serve to put us upon our guard concerning the admission of new simple metals, which, on close examination, will often, as in the present case of the palladium, turn out to be combinations of so close a nature as not to be easily decomposed. A great obstacle to the discovery of this deception is, no doubt, the little dependence that is to be placed on specific gravities; since, as we have seen above, a contrary anomaly to that which operates upon platina and mercury may take place in other alloys, which in some cases become as much heavier than the mean as the palladium becomes lighter. In a word, the principal task of modern chemists seems to be to simplify and reduce the immense number of supposed elements; and, by a close observation of nature, to learn from what a small store of primitive materials all that we behold and wonder at has been originally created.

An Account of the sinking of the Dutch Frigate Ambuscade, *of* 32 *Guns, near the Great Nore; with the Mode used in recovering her. By Mr.* Joseph Whidbey, *Master Attendant in Sheerness Dock Yard. Communicated by the Right Hon. Sir* Joseph Banks, *Bart. K.B. P.R.S.* Read April 28, 1803. [*Phil. Trans.* 1803, *p.* 321.]

On the 9th of July 1801, this frigate sailed from Sheerness harbour with a strong favourable wind. In about thirty minutes she went down by the head, and in less than four minutes more she sunk entirely, twenty-two of the crew having perished by the fatal accident, which is ascribed to the hawse-holes being extremely large, low, and carelessly left open; so that by the crowd of sail the ship bore, they were pressed under water, by which means she filled imperceptibly, and sunk before any preventive means could be applied.

In order to raise her, Mr. Whidbey had recourse to another Dutch vessel of 1063 tons burthen, and four lighters of 100 tons each. In what manner he slung the frigate through the ports, and across the decks, and by proper purchases and other expedients first overcame the powerful effect of cohesion at the bottom, and then having floated her brought her safe into harbour, and completely recovered her, can only be clearly understood by perusing the paper, and at the same time inspecting the drawing which accompanies it. Mr. Whidbey, at the end of his paper, acknowledges that he does not apprehend that there is anything new in the mode he has adopted in weighing this frigate, unless it be the expedient he had recourse to in order to remove the effect of cohesion: and he declares his opinion, that if a similar principle had been applied in the attempt made to weigh the Royal George, it would most probably have succeeded.

Observations on a new Species of hard Carbonate of Lime; also on a new Species of Oxide of Iron. By the Count de Bournon, *F.R.S. and L.S.* Read May 26, 1803. [*Phil. Trans.* 1803, *p.* 325.]

The new carbonate of lime here treated of was first noticed in a group of hexahedral pyramidal crystals in Mr. Greville's collection. The Count, observing that the exterior appearance of these crystals was very different from that which is peculiar to carbonate of lime, endeavoured in the first place, in order to determine their nature by their configuration, to reduce them, by splitting, into the rhomboidal form of this substance; but he soon found, to his surprise, that not only he failed in producing such a fracture, but that it was with the greatest difficulty he could break them into any regular form whatever. Their hardness he found to be much greater than that of common carbonate of lime, being such as would scratch very easily the fluates of lime, and even glass. Their specific gravity was 2912. They were colourless, and in general perfectly transparent. When heated they are slightly phosphorescent. They dissolve quickly, and with great effervescence, in nitric acid.

After various attempts, he found at length that these crystals would admit of being divided into two directions, tending to produce a rhomboidal tetrahedral prism. A great part of the paper is taken up in describing the angles produced by these fractures, and also the figures of all the different crystals he had opportunities of observing; which descriptions are illustrated by drawings.

This substance, upon further inquiry, does not appear to be very scarce, there being no less than twelve specimens in Mr. Greville's collection, most of which came from Carinthia and Transylvania, and some from Scotland. The delicate stalactitical substance, hitherto known by the name of *flos ferri*, belongs to this species of carbonate, of which the crystals, though ever so minute, have so singular a degree of hardness as to resist the common efforts of the fingers to break them.

The matrix of this kind of carbonate is generally a brown oxide of

iron, mixed with a portion of argill. Its appearance altogether might easily induce mineralogists to refer it to that kind which has been distinguished by the name of Arragonite; but various reasons are here assigned for considering them as distinct substances. Chemical analysis has not yet afforded any criteria for distinguishing this species either from the arragonite or the common carbonates of lime. And it is lastly thrown out, merely as a conjecture, whether its greater degree of hardness and specific gravity may not be owing merely to its constituent particles being more minute and more closely connected.

The second part of this paper, which treats of a new species of oxide of iron, is prefaced by a short survey of the different appearances of the oxides of iron, according to their various degrees of combination with oxygen: and it is here suggested, that not only the proportion of oxygen, but also the mode of combination, may contribute to produce varieties which cannot by any other means be accounted for. The new species here described is thought to hold its place between the slightly attractable oxide of iron called Specular ore, or, by Abbé Hauy, *fer oligiste*, and that kind which no longer crystallizes except in a very indeterminate form. Its surface is of a gray colour, and has a specular appearance: it is not at all acted upon by the magnet, and seems to be the last degree of oxidation in which iron retains the property of crystallizing in a regular form. This form is a perfect cube; its fracture is conchoidal; its hardness is rather inferior to that of the slightly attractable oxide of iron; its specific gravity only 3961.

To this species it seems ought to be referred the *Eisen-glimmer* of the Germans, when it is not attractable. When at all acted upon by the magnet, it ought to be numbered among the slightly attractable oxides of iron. Specimens of this new species have been brought from Lapland, and are often found mixed or embodied in other oxides of the same metal.

The author, lastly, points out how, by the red colour of the powder and by scratching, this species may be, in a general way, distinguished from the others; an object of no trivial importance, since the products of metal from the ores may be influenced by this discrimination.

Account of the Changes that have happened, during the last Twenty-five Years, in the relative Situation of Double-stars; with an Investigation of the Cause to which they are owing. By William Herschel, *LL.D. F.R.S.* Read June 9, 1803. [*Phil. Trans.* 1803, *p.* 339.]

After some general observations on the various discoveries lately made, which have contributed to extend our knowledge of the construction of the heavens, Dr. Herschel assumes as a possibility, that among the multitude of the stars in the firmament, there may be instances of pairs of stars of the same or different relative magnitudes, which may revolve, either in circles or ellipses, round their common

centre of gravity. Having started this hypothesis, he proceeds to give an account of a series of observations on double stars, comprehending a period of twenty-five years, which, in his opinion, will go to prove, according to the above assumption, that many of them are not merely double in appearance, but must be allowed to be real binary combinations of two stars, intimately held together by the bond of mutual attraction.

In this inquiry, three bodies or points are to be exclusively attended to:—1st, the largest of the two stars which make up the double star; 2nd, the smaller of these two stars; and 3rd, the place of the sun, which at the great distance of the fixed stars may be taken for that of the observer. It is obvious, that as this investigation must be conducted in an hypothetical manner, it is necessary to consider the appearances that would be produced by the motion of these three bodies, either singly, by pairs, or collectively. This renders it necessary to contemplate a variety of cases, and previously to lay down a certain theory to which the observations may be referred upon the supposition of either of those cases. For this purpose the author has drawn up tables, showing the appearances as to distance and angle of position that must result from the motions of either of those three bodies, whether in right ascension or declination.

As the number of double stars in which he has ascertained manifest changes in their relative positions amount already to more than fifty, our author thinks it advisable to confine himself in this paper to only a few of those instances; and, accordingly, we find here an investigation of the changes of six double stars, viz. α Geminorum, γ Leonis, ϵ Bootis, ζ Herculis, δ Serpentis, and γ Virginis; reserving himself to treat of the others in a future paper.

His observations on the double star α Geminorum commenced so long ago as the year 1779, and have been regularly continued to the present time. In this interval of twenty-three years and a half, the distance of the two stars has not varied, being constantly about two diameters of the large star; but the angle of position has altered considerably. In the year 1779 it measured 32° 47′ north preceding; and by the last observation in the present year it is now only 10° 54′; so that in the space of twenty-three years and a half it has manifestly undergone a diminution of no less than 21° 54′: and the intermediate observations leave no room to doubt that this change has been the effect of a gradual and regular motion.

A revolving star, it is evident, would at once explain such a progressive change in the angle of position, without an alteration of the distance; but this being a supposition of which we have not hitherto any precedent, it ought certainly not to be admitted without the fullest evidence. Accordingly, our author enters into a minute examination, founded on geometrical and optical principles, whether the above-mentioned phænomena cannot be satisfactorily explained by other motions of the stars or of the sun, according to the several hypotheses above indicated, with the addition of others in which the two stars are not supposed to be at equal distances from the sun.

Each of these hypotheses having been carefully investigated, it is found that all except that of a rotatory motion of the small star round the large one, or of their joint motions round a common centre of gravity, offer difficulties which cannot be surmounted. Adopting, therefore, this last-mentioned hypothesis as the true one, our author proceeds to a more detailed examination of the several angles of position he has taken in the course of his observations; and having also determined those angles by calculation from the annual rate of 56′ 18″, deduced from the total change in the whole period, he finds the agreement so obvious as hardly to admit of a doubt of this being the true mode of accounting for these variations. He has, moreover, the satisfaction of being able to quote an observation of Dr. Bradley, made in the year 1759, which he lately obtained from Dr. Maskelyne; according to which, the two stars of α Geminorum were in that year seen in an angular position, exactly corresponding with the situation they ought to have been in, according to the rate here assigned for their rotatory motion: so that the time of a periodical revolution may now be calculated from an arch of 45° 30′, which has been described in forty-three years and a half.

Thus, from the great regularity of this motion, he now thinks himself authorized to conclude, that the orbit in which the small star moves round the large one, or perhaps the orbits in which they both move round their common centre of gravity, are nearly circular, and at right angles to the line in which we see them; and that the time of a whole apparent revolution will be about 342 years 10 months.

We shall not presume to enter here upon any detail of the investigations respecting the five other double stars mentioned in this paper, any further than to record the general results deduced from them.

In γ Leonis the plane of the orbit of the small star is found not to be at right angles with the line of vision, the distances having varied considerably since the commencement of the observations, and these different distances affording the elements of an ellipsis which will explain the appearances, although the orbit be in fact, or nearly, circular. The periodical time of this revolution is calculated at about 1673 years.

As to ε Bootis, the changes observed during twenty-two years indicate that one of the periodical revolutions cannot take up much less than 1681 years; but as the figure and situation of the orbit cannot as yet be accurately determined, some uncertainty still remains even concerning this period.

The observations on ζ Herculis afforded a phænomenon hitherto unknown in astronomy; namely, an occultation of one star by another. Whether this be owing to solar parallax, to proper motion, or to the motion of one of the stars in an orbit whose plane is nearly coincident with the visual ray, is not as yet determined; nor is any periodical time hitherto assigned to it.

The periodical time of δ Serpentis is calculated at 375 years, and that of γ Virginis at 705 years. Their distances have not varied for

many years back; whence it may be inferred that the planes of their orbits are really or nearly at right angles with the visual ray.

Dr. Herschel being aware that the observations he brings forward in this paper are of a nature so delicate and minute as to afford opportunities for cavil, has been at considerable pains to point out the principal circumstances that contribute to the perfection of telescopes and micrometers, and the precautions to be used as to the state of the atmosphere. Those ought, he thinks, to be particularly attended to by accurate observers.

An Account of the Measurement of an Arc of the Meridian, extending from Dunnose, in the Isle of Wight, Latitude 50° 37′ 8″, *to Clifton, in Yorkshire, Latitude* 53° 27′ 31″, *in course of the Operations carried on for the Trigonometrical Survey of England, in the Years* 1800, 1801, *and* 1802. *By Major* William Mudge, *of the Royal Artillery, F.R.S.* Read June 23, 1803. [*Phil. Trans.* 1803, *p.* 383.]

In this paper we are presented with a further continuation of the several accounts given, ever since the year 1785, of the trigonometrical surveys carried on over various parts of the kingdom. Having now proceeded a great way in these surveys, Major Mudge thought it high time to attempt the measurement of a considerable arc of the meridian in our latitudes. He first assigns his reasons for preferring the meridian he has here adopted, which depend chiefly on the nature of the country being less hilly and liable to less obstruction than any other tract of the length of this arc in the island. He fixed on Dunnose, in latitude 50° 37′ 8″, for the southern extremity, and on Clifton, a small village in the vicinity of Doncaster, latitude 53° 27′ 31″, for the northern termination of this arc: and near the latter place he found a convenient plain, viz. Misterton Carr, for the measurement of a base of verification.

As the accuracy of the zenith observations would be most essential towards the perfection of this measurement, a new zenith sector was constructed for the purpose by Mr. Ramsden, and finished by Mr. Berge, the excellence of which is here attested by its being called the first instrument of its kind. Its merits consist chiefly in the means of uniting the sectorial tube to its axis, so as to insure the permanency of the length of its radius when erected for observation; in a more accurate method of adjusting the instrument vertically; an easy way of placing the face of its arch in the plane of the meridian; and a contrivance by which the plumb-line can be brought precisely over the point marking the centre of the circle, of which the divided arch of the sector should be a part.

Having given a minute description of this instrument, and of the preparatory operations for the series of observations to be made with it, such as the construction of a proper observatory, and other auxiliary requisites, the author proceeds to give us, in different tables,

the angles of a number of triangles observed near the north end of the arc; the calculations of the sides of a series of triangles extending from Dunnose to Clifton; and, from these data, a calculation of the meridional distance between Dunnose and Clifton. Then come the observations with the zenith sector at Dunnose, Clifton, and Arbury Hill, near Daventry, a point almost in the middle of the arc, which was chosen with a view to ascertain how far the observations at the terminations would agree with others made for finding the value of its parts. A few additional observations made at the Royal Observatory are also given, serving to demonstrate the precision of the former ones, and the accuracy of the instrument.

Next follow the extensive and laborious calculations by which, in order to assimilate the numerous observations made at different times, they are all reduced, from the respective days on which they were made to the 1st of January 1802: the equations here introduced are those for aberration, nutation, semi-annual solar equation, precession, and refraction.

The general conclusions deduced from this ample stock of observations and calculations are, that the whole arc, subtending an angle of 2° 50′ 23″, measures 1,036,337 feet; so that the length of a degree on the meridian, in latitude 52° 2′ 20″, is = 60·820 fathoms. This degree, at the latitude of Arbury Hill, is found to be 42 fathoms longer; whereas, admitting the earth to be an ellipsoid, with the ratio of its axis as 229 to 230,* it should be 10 fathoms less. On maturely weighing all the causes that may have occasioned this deviation, it is thought most likely that, owing to different attractive forces, which increase as we proceed northward, the plumb-line of the sector has been drawn somewhat towards the south at each of the stations,—a circumstance that must be carefully attended to in the prosecution of this survey, whenever the zenith sector is to be used. It is observed in general, that meridional observations carried on in insular countries are not so likely to afford just conclusions, with regard to the different lengths of the degrees, as the same operations if conducted in places very remote from deep seas.

Adverting, lastly, to the operations of the French astronomers who have measured the arc of the meridian between Paris and Barcelona, which distance was found = 3,527,921 English feet, this, combined with the arc lately measured, gives the whole meridional distance between Clifton and Barcelona, being 12° 5′ 42″·79, something more than the thirtieth part of the whole circumference of the globe, = 4,411,968 feet. According to this determination, the mean length of a degree of the meridian, in latitude 47° 24′, will be = 60,795 fathoms; and in the latitude 51° 9′, the degree will measure 60,825 fathoms.

In an Appendix are subjoined the latitudes and longitudes of those places intersected in the surveys of Essex, Suffolk, &c. whose distances from their respective places of observation are given in the Philosophical Transactions for 1800; which, it is asserted, cannot

but be highly useful, as they may be depended on; the interior surveys of those parts having since proved that no erroneous intersections had been made in those operations.

The Bakerian *Lecture. Experiments and Calculations relative to physical Optics. By* Thomas Young, *M.D. F.R.S.* Read November 24, 1803. [*Phil. Trans.* 1804, *p.* 1.]

It consists of six sections, the first of which is intended to convey an experimental demonstration of the general law of the interference of light. This demonstration rests on two experiments, the results of which are brought in proof, that fringes of colours, and even the crested fringes described by Grimaldi, are produced by the interference of two portions of light. These results are, that if one of the two edges of a shadow produced by a narrow opake body be intercepted by a screen at a small distance from that body, the opposite edge will no longer exhibit the fringed appearance which it had in common with the former edge, when the latter was not intercepted.

Under the second head we have a comparison of measures of the intervals of disappearance of light when refracted between two edges of knives, or intercepted by a hair or a thin wire. The experiments, which were partly suggested by some observations of Sir Isaac Newton, are here collected in tables: and the author states, as a general inference, that if we thus examine the dimensions of the fringes under different circumstances, we may calculate the differences of the lengths of the paths of the portions of light which have been proved to be concerned in producing those fringes; and we shall find, that where the lengths are equal, the light always remains white; but that where either the brightest light, or the light of any given colour, disappears and reappears a first, a second, or a third time, the differences of the paths of the two portions are nearly in an arithmetical progression.

In the third section, these principles are applied to explain the repetition of colours sometimes observed within the common rainbow, particularly those described in the Philosophical Transactions by Dr. Langwith and Mr. Daval. The train of reasoning here adduced would lose too much of its evidence by being abridged.

The fourth section is entitled, "Argumentative inference respecting the Nature of Light." Here we meet with something of a controversial nature, in which those who have adopted theories different from that which our author is desirous to establish, are called upon to explain his experiments according to their principles. What appears to him to operate chiefly against the advocates for the projectile hypothesis of light, is, that light moves more slowly in a denser than in a rarer medium, and that hence refraction is not the effect of an attractive force directed to a denser medium.

The fifth section treats of the colours of natural bodies. The nature of the light transmitted by various bodies is here described, but

more particularly that which passes through blue glass. This, we are told, may be separated by the prism into seven distinct portions, nearly equal in magnitude: the two first are red, the third yellowish green, the fourth green, the fifth blue, the sixth bluish violet, and the seventh violet. This division, it seems, agrees perfectly with that of the light reflected by a plate of air $\frac{1}{16,480}$th part of an inch in thickness: and hence we may infer the extreme minuteness of the particles of light.

The sixth and last section describes an experiment on certain dark rays, which were first noticed by Ritter, and relates to the existence of solar rays accompanying light, but cognizable only by their chemical effects. This fact our author has confirmed by observing the effect of the reflection of these invisible solar rays from a thin plate of air capable of producing the well-known rings of colours. This image he threw on paper dipped in a solution of nitrate of silver, and in less than an hour he distinctly perceived portions of three dark rings, nearly of the same dimensions, but manifestly different from the coloured rings. This seems to coincide with Dr. Herschel's late discovery of rays of invisible heat; but our author doubts whether we are yet possessed of thermometers of sufficient delicacy to place implicit confidence in the experiments hitherto made on these rays by means of that instrument.

Continuation of an Account of a peculiar Arrangement in the Arteries distributed on the Muscles of slow-moving Animals, &c. In a Letter from Mr. Anthony Carlisle *to* John Symmons, *Esq. F.R.S.* Read December 8, 1803. [*Phil. Trans.* 1804, *p.* 17.]

Since the communication of his former paper on that subject, the author has collected further illustrations respecting the connexion between the disposition of the blood-vessels and the actions of the muscles. His first observations relate to the spermatic and intercostal arteries, and those of the diaphragm in men; which, he finds, are distributed in a different manner from those of the ordinary muscles. Compared with the distribution of the coronary arteries, it is found that the latter are much more subdivided or arborescent than any other set, and that accordingly these supply the heart,—a muscle whose actions we know are more rapid than those of any other part of the muscular system.

It is hence inferred, that any impediment to the accustomed course of the blood, flowing through muscles, induces a corresponding diminution in their power of action; and that wherever we find cylindrical arteries emitting few lateral branches, we may conclude that they appertain to muscles of slow but in general of long-continued motion. Of this, instances are given in the human eye, the swimming-bladder of fishes, the intestinum ~~ileum~~ of the Cavia Aguti, and various animals of the amphibious class. The better to illustrate his observations, the author has added figures of the swimming-bladder of the tench, and of the ~~ileum~~ of the Aguti.

An Account of a curious Phænomenon observed on the Glaciers of Chamouny; together with some occasional Observations concerning the Propagation of Heat in Fluids. By Benjamin Count of Rumford, *V.P.R.S. Foreign Associate of the National Institute of France, &c. &c.* Read December 15, 1803. [*Phil. Trans.* 1804, *p.* 23.]

The fact here stated is as follows:—At the surface of a solid mass of ice, of vast thickness and extent, viz. the Glaciers of Montanverd, certain pits are frequently met with, about seven inches in diameter, and more than four feet deep, perfectly cylindrical, and always quite full of water: their sides are smooth, or rather polished, and their bottoms hemispherical and well defined. They are always found on the level parts of the ice, and only in the summer season, increasing gradually in depth as long as the hot weather continues, and disappearing at the return of winter, when they are completely frozen up.

After calling upon those who maintain that water is a conductor of heat, to solve this phænomenon according to their principles, and pointing out to them, that as the water in these pits, being surrounded by ice, must continually be at the freezing point of temperature, it is not the general heat of the fluid that can melt the ice at the bottom of the pits, our author proceeds to give the following explanation of this singular effect.

The warm winds, he says, which in summer blow over the surface of this column of ice-cold water, must evidently communicate some small degree of heat to those particles of the fluid with which this warm air comes into immediate contact; and the particles of the water at the surface so heated, being rendered specifically heavier than they were before by this small increase of temperature, sink slowly to the bottom of the pit; and here they come in contact with the ice, and communicate to it the heat by which the depth of the pit is continually increased.

Count Rumford mentions next the singular but well-authenticated fact, of the equal temperature, at all seasons, of the water at the bottom of lakes; and shows how difficult, if not impossible, it must be to explain this phænomenon on a supposition of water being a conductor of heat. With a view to illustrate this subject, he gives us hopes that he will soon favour us with some observations, showing why all changes of temperature in transparent liquids must necessarily take place at their surfaces. Some further strictures are next given, and certain difficulties are pointed out, on the cause of the descent of heat in liquids. And, lastly, notice is taken of the observations of Mr. Thompson of Edinburgh, on the experiments our author had contrived to render visible the currents into which liquids are thrown on a sudden application of heat or cold. The whole of this discussion rests on the accuracy of his observations, which Mr. Thompson had called in question, but in which he confidently asserts there was no fallacy whatever.

Description of a triple Sulphuret, of Lead, Antimony, and Copper, from Cornwall; with some Observations upon the various Modes of Attraction which influence the Formation of mineral Substances, and upon the different Kinds of Sulphuret of Copper. By the Count de Bournon, *F.R.S. and L.S.* Read December 22, 1803. [*Phil. Trans.* 1804, *p.* 30.]

The copious contents of this paper are arranged under the three following heads:—1. A description of the sulphuret of lead mentioned in the title; 2. Observations on the various modes of attraction which influence the formation of mineral substances; and 3. Observations upon the different kinds of sulphuret of copper.

1. The cupro-antimonial sulphuret of lead, described in the first part, has hitherto been found only in Cornwall; and though many specimens of it are to be met with in various collections in the kingdom, yet no writer has hitherto taken any particular notice of it, nor has it been classed by any of the late compilers of mineralogical systems. Mr. Hatchett is the first who, on a careful analysis, has ascertained it to be a triple sulphuret, in which the sulphur is combined with lead, antimony, and copper; each of these ingredients exhibiting their characters in so striking a manner as to afford, in some measure, a new example of a natural compound in the mineral kingdom.

The following are its principal characters.—It is of a dark gray colour; it has a brilliant lustre, and is very brittle; its hardness is such, that it very easily cuts calcareous spar, but is not sufficient to scratch fluor spar; it slightly marks white paper; when rubbed, it does not emit any smell; when powdered and thrown upon a hot iron, it emits a phosphorescent light; and its specific gravity is 5765. The form of its primitive crystal is a rectangular tetrahedral prism, with terminal faces perpendicular to its axis; but as no specimen has yet been discovered in which the above-mentioned form is totally destitute of secondary facets, the author enters into a minute investigation of the various modifications of this form, hoping by this means to promote essentially the knowledge of the crystalline character, so important in the study of mineralogy. These modifications are four in number, and can only be understood by inspecting the figures which are subjoined to the paper.

To these characters is added the more essential one, which is supplied by the proportions of the constituent parts of the substance. These, according to Mr. Hatchett's analysis, consist of 42·62 of lead, 24·23 of antimony, 12·80 of copper, and 17 of sulphur: 1·20 of iron was likewise yielded in the process; but this is thought to have been a mere accidental mixture. It is next observed, that all the characters in this substance indicate very plainly the mutual combination of the three sulphurets of which it is found to be composed; the whole of the external characters above described differing materially from those of either of the three sulphurets, and also from those of any metallic substance hitherto known; and the pro-

portion of sulphur obtained from it by analysis being exactly the total quantity that should be contained in the three sulphurets of which this substance consists. The specific gravity of this substance, compared with those of its ingredients, also indicates that the combination is attended with an expansion nearly proportionate to the numbers 6000:5765.

2. The author, proposing to avail himself of the opportunity afforded him by this inquiry, to enter into an investigation concerning the various ores that are produced by the combination of sulphur and copper, of the nature of which neither mineralogy nor chemistry has yet supplied us with any certain information, thinks it necessary, in the second part of his paper, to offer some remarks concerning the different modes of attraction that appear to influence the formation of mineral substances.

Two kinds of attraction have hitherto been admitted to prevail in the formation of mineral substances, viz. the attraction of *composition*, and the attraction of *aggregation*. The former, which is more generally known by the name of chemical attraction, takes place only between the most simple or primitive molecules of a substance; which, however, must be of dissimilar nature: and to its action is owing the formation of new, or, as they may properly be called, secondary or integrant molecules; because they, and they only, determine the nature of all the compound bodies belonging to the mineral kingdom. The difference existing between mineral bodies is now said to depend—1st, Upon the nature of the primitive molecules, by the combination of which they are produced; and 2ndly, Upon the proportion in which these molecules, supposing them to be the same, are combined together. The combination of these secondary molecules is effected by the attraction of aggregation, which unites them into one or several masses, perfectly homogeneous in all their parts. This attraction of aggregation seems to be susceptible of various modifications, which alter its manner of acting upon the constituent molecules. Of these, two are here mentioned:—1. The crystalline attraction of aggregation; and 2. The simple attraction of aggregation. The former always takes place between similar molecules, and is either *regular*, *irregular*, or *amorphous*. The first of these produces solid bodies, which are either constantly of the same form, or subject to certain laws of variation, which are always capable of being referred to the same primitive form. This, like all other crystalline attractions, can only take place in fluids, which, among other conditions, must be at rest when it is operating. When the fluid happens to be agitated, the crystallization will then be of the second kind, and the forms produced will be irregular: and when the agitation of the fluid is still greater, small irregular detached masses will subside, and unite together by a mode of attraction, which is here called *simple homogeneous attraction of aggregation*, of which instances are given in the granulated quartz and granulated carbonate of lime. This attraction operates at times simultaneously with the simple homogeneous attraction; and then the granulated masses, instead of

being composed of an aggregate of irregular grains, will appear to consist of small crystals of a very regular form, as may be very frequently seen in manganesian carbonate of lime. At other times, again, the molecules, instead of uniting together by the influence of the crystalline attraction of aggregation, are precipitated in a detached but confused manner; and then a simple aggregation takes place, which banishes all appearances of crystallization, and affords the aspect which has been distinguished by the name of *compact* or *earthy*.

This last, or the amorphous species of crystallization, is here described as being that kind which, though it produces no determinate form, is nevertheless the result of a regular aggregation. This is thought to depend on the peculiar form of the primitive molecules; such as, for instance, the globular, or others approaching to it. Calcedony, girasol, and pure transparent steatite, appear to owe their origin to this mode of formation. And here the author enters into a minute disquisition concerning the cause of the deception which has often taken place and has led several experienced mineralogists to ascribe to these substances, especially the chalcedony, a certain degree of regular crystallization.

Besides these attractions between similar molecules of substances, there exists another, between such similar molecules and others which are *dissimilar*, or of a different nature. This introduces what may be called an *heterogeneous attraction of aggregation*, which is much weaker and more variable than any of the others, and produces substances which can no longer be called chemical combinations. In these, the dissimilar ingredients may, and often do, vary in proportion, according to the different circumstances under which they are formed; as, for instance, in those kinds of tremolite which have the dolomite for matrix where the carbonate of lime is in the proportion of $\frac{18}{100}$; whereas in those kinds which have an argillaceous matrix, it is only in the proportion of $\frac{8}{100}$.

Although these heterogeneous compounds be not attended with any changes in the chemical nature of their ingredients, yet they frequently, as has already been observed, admit of an alteration in their physical construction, and very often induce variations in such of their characters as more immediately depend upon that construction; such as, their specific gravity, their hardness, their transparency, and even (particularly in the class of stones) their colour. Hence it appears that the mineralogist cannot pay too much attention to this mode of attraction; since, by a due application of it, he will be enabled to understand the accidental causes of the variations to which those substances are liable. Nor should the chemist be less mindful of such an agent; since, by neglecting that precaution, he will be constantly exposed to confound those products which really belong to the chemical composition of the substances which he examines with those which are foreign to it.

At the close of this part of the paper, the author acknowledges that the different kinds of attraction here described may be nothing

more than simple modifications of one and the same power, originally belonging to matter; but he maintains, at the same time, that these modifications certainly exert as much force, at the time of their operation, as could be exerted by attractive forces that are really different.

In the third part, which treats of the different kinds of sulphuret of copper, we are first apprised of the importance of this inquiry, by the observation, that, on comparing the different characters of this substance with those of the triple sulphuret described in the first part of this paper, we shall find that the antimony and the lead, which have been generally considered as constituent parts of the last-mentioned ore, are, in fact, nothing more than accidental ingredients, introduced merely by the attraction of aggregation.

In the sequel of the paper, the author introduces an ample stock of observations and analyses respecting various cupro-sulphurets, which have been hitherto very imperfectly examined. And first he treats of the gray tetrahedral sulphuret of copper, which is found to consist of copper, iron, and sulphur, in different proportions; the gray copper ore, called *Fahlerz* by the Germans, being of the same composition. The yellow copper ore, or *Kupferkies* of the Germans, appears to be double sulphuret of copper and iron, but constituting a species distinct from the gray sulphuret of the same tetrahedral form. Among these, there is one in particular which has not yet been described as belonging to this ore; namely, the dodecahedron with rhombic planes, which has hitherto been found only in Cornwall, and there only in small quantities, though in crystals of considerable size. It is also shown at length wherein most of the characters of the tetrahedral yellow cupro-martial sulphuret differ from those of the octahedral sulphuret of iron.

The following question is next proposed, and strongly recommended to the future investigation of mineralogists. As the true sulphuret of copper and the fahlerz are of a blackish gray colour, how comes it that the kind of cupro-martial sulphuret, commonly called pyritical copper, has always that brilliant yellow colour which particularly characterizes it, and which is the principal cause that leads many mineralogists to consider it as being nothing more than a martial pyrites mixed with copper? The solution of this difficulty, we are told, will materially contribute to ascertain the external characters of this kind of sulphurets.

The cupro-martial sulphuret, called by Werner *Buntkupfererz*, is a new species of this ore, which crystallizes in forms that are peculiar to it, and not at all analogous to those of the other cupro-martial sulphuret. The ingredients, which are solely copper, iron, and sulphur, differ considerably in their proportions in different specimens.

The author desires that what he has here said may be considered merely as a cursory account of some of the sulphurets of copper, from which, however, he thinks it may be inferred, that there exists a great number of species of this substance which have not yet been de-

scribed, and that many of those with which we are acquainted have not been sufficiently examined.

A general observation, which he tells us is founded upon long experience, is, that there exists a great variety of minerals which have the same substance, or collection of substances, for their basis, and are combined with the same modifying substance, but whose differences arise merely from the variety of proportions in these bases or substances.

The paper closes with an earnest exhortation to those who cultivate mineralogy, to choose for the subjects of their experiments a variety of perfect specimens from different districts, and as much as possible from different matrices; that they make a number of comparative analyses; and that the mineralogist and the chemist mutually sanction the operations of each other in their respective departments.

Analysis of a triple Sulphuret, of Lead, Antimony, and Copper, from Cornwall. By Charles Hatchett, *Esq. F.R.S.* Read January 26, 1804. [*Phil. Trans.* 1804, *p.* 63.]

This is the analysis to which the Count de Bournon more than once refers in his elaborate account of the same mineral, lately read to the Society. We find here, in addition to the information contained in that paper, that one of the reasons why this very scarce ore has been hitherto so little attended to, is probably its great resemblance to an ore of antimony; that by all the chemical tests by which it has been tried, its constituent parts are manifestly lead, antimony, copper, and a small proportion of iron, the whole combined with sulphur; and that when the specific gravity, the external and internal colour, the fracture, the grain, and other characters here described are considered, there can be no doubt that the three first metals exist in the ore in, or nearly in, the metallic state, combined with sulphur, so as to form a triple sulphuret. The proportion of the ingredients are as given by Count de Bournon, who, in fact, took them from this paper.

Observations on the Orifices found in certain poisonous Snakes, situated between the Nostril and the Eye. By Patrick Russell, *M.D. F.R.S. With some Remarks on the Structure of those Orifices; and the Description of a Bag connected with the Eye, met with in the same Snakes. By* Everard Home, *Esq. F.R.S.* Read February 2, 1804. [*Phil. Trans.* 1804, *p.* 70.]

The orifice, which is the principal object of this paper, has been long since noticed by naturalists, who conceived it to be the external organ of hearing. Dr. Russell, in the many opportunities he has had of observing a variety of snakes, has particularly examined them with respect to this feature; and he here informs us, that he has found in the whole class (exclusive of the rattle-snake,) fifteen or

sixteen species of Coluber, and three of the genus Boa, which have these lateral orifices; that they have not as yet been discovered in the genus Anguis; and that in general it appears that only venomous snakes have this distinctive character.

From Mr. Home's description and remarks, we learn that these orifices do not lead to the nostril or to the ear, but to a distinct bag of a rounded form, there being within the skull a hollow of the same shape, surrounded by bone, which seems purely intended to receive it. This cavity is described as resembling a cup, formed by the bones of the skull and those of the upper jaw, and not unlike the orbit. The bags bear a relative proportion to the size of the snake; they are, like the eyelids, lined with a cuticle, which forms the transparent cornea, making a part of the outer cuticle; both which, it seems, are shed at the same time.

Mr. Home proceeds next to a description of similar bags in the deer and antelope kinds, which were by some thought to be lachrymal glands or ducts. On close examination, however, it is found that these bags have a secretion of their own, and that there is no reason for thinking that tears ever pass into them, the passage into the nose being unusually free, and the orifices of the bags in general unfavourably situated for the reception of the tears. The use to which the fluid secreted in these bags is applied, is as yet unknown. In the snake this apparatus has that position which seems best adapted to pour out the fluid upon the cornea when the head of the snake is in an erect position.

An Enquiry concerning the Nature of Heat, and the Mode of its Communication. By Benjamin Count of Rumford, *V.P.R.S. Foreign Associate of the National Institute of France, &c.* Read February 2, 1804. [*Phil. Trans.* 1804, *p.* 77.]

The importance of the investigation here entered into,—inasmuch as it applies to most of the operations of nature as well as art,—appears so manifest, that we shall not recapitulate what the author advances on that subject. Before he proceeds to the detail of his experiments for the purpose of computing the emissions of heat from various bodies under a variety of circumstances, he finds it necessary to premise a minute description of the principal part of the apparatus he contrived for his purpose. This instrument consists of a hollow cylindrical vessel of brass, four inches long, and as many in diameter. It is closed at both ends; but has at one end a cylindrical neck about eight-tenths of an inch in diameter, by which it is occasionally filled with water of different temperatures, and through which also a thermometer, constructed for the purpose, is occasionally introduced, in order to ascertain the changes of temperature in the fluid. As it was in the first instance only meant to observe the quantity of heat that escapes through the sides of the vessel, two boxes were contrived, filled and covered with non-conducting substances, such as eider-down, fur, &c., which were fitted to the two ends or flat surfaces of

the cylinder. Six of these instruments, with proper stands, and auxiliary implements of obvious construction, were prepared for the sake of comparative experiments.

A previous trial was made with two of the cylinders, the vertical polished sides of the one being naked, and those of the other covered with one thickness of fine white Irish linen, strained over the metallic surface. Here it was found, contrary to expectation, that in a certain space of time the covered cylinder had lost considerably more heat than the naked one.

In reflecting on this experiment it occurred to the author, that in order to insure the accuracy of the comparison between experiments made at different times and at different places, it would be necessary to fix on some particular interval of the scale of the thermometer above the temperature of the air by which the instrument is surrounded. He therefore determined that all experiments should begin at the temperature of 50°, and end at 40° above that of the surrounding atmosphere, an interval of 10° appearing to him sufficient for the purpose of his investigation. Finding also that most experiments would take up several hours, during which he could often not be present to observe the thermometer at the different points which ought to be ascertained, and observing that the rate of cooling of hot bodies afforded a pretty regular progression, he determined to investigate this rate more minutely, with a view to obtain the means of introducing such interpolations as would complete the series of observations. Accordingly, on a given line, on which were set off the times of cooling, he applied ordinates representing the different temperatures corresponding to those times; and having joined the opposite terminations of these ordinates, he had the satisfaction to find that this latter connecting line was in fact the logarithmic curve, by means of which he would be enabled to supply by computation any intermediate points which happened to have been neglected during the observation. The problem according to which these interpolations are to be computed, is given at full length.

These previous precautions and expedients having been fully stated, the author proceeds next to the enumeration of his long series of experiments, the first of which is merely the comparison, which has already been mentioned above, between the naked and the covered cylinders. The result was, that the former was 55′ in cooling 10°, while the latter cooled through the same interval in $36\frac{1}{2}'$; whence it appears that clothing does in some instances expedite the passage of heat out of a hot body instead of confining it. The only mode in which it is thought that this unexpected result can be accounted for, is by admitting that, as air is known to adhere with considerable obstinacy to the surfaces of certain solid bodies, the particles of air which were in immediate contact with the surface of the naked cylinder were so attached to the metal as to adhere to it with considerable force; and as confined air is known to be a very warm covering, it seems probable that the retardation of the cooling in this vessel was owing to that invisible covering, the air in contact with

the other vessel being absorbed, displaced, or in a great degree driven away by the colder covering of linen which closely embraced it.

Led on by this conjecture, several experiments were made with cylinders covered with one, two, or more coatings of glue, and of copal varnish; and the results, in fact, turned out favourable to the supposition, the cylinder with one coat of glue losing 10° of its heat in 43′, and that with two coatings in about 38′. With the copal also the cooling of the instrument became more and more rapid as the thickness of the varnish was increased; till, however, eight successive coatings having been applied, the cooling again became less rapid, and it was found that there was a maximum of thickness which produced the greatest effect. No probable reason is yet assigned for this limitation.

The next object was to find out what effect colour would produce in the experiments, and accordingly the cylinder with eight coatings of varnish was painted black; it was also painted in the same manner after all the varnish had been washed away; and lastly, it was likewise painted white: in each of these instances the cooling was accelerated by the paint, nearly in the same proportion as in the preceding experiments.

A nicety occurred now in the conducting of the experiments, which was thought to deserve particular attention: though the apparatus for confining the heat at the two flat ends of the cylinder was the best that could be contrived, yet it is not at all unlikely that some would escape in those directions, and thus occasion some fallacy in the results.

In order to investigate this point, a given number was previously assumed as the measure of the whole quantity of heat emitted by the whole instrument, without terminal coverings, during a certain period. The surface of the whole of the cylinder was then accurately measured, and also that of its vertical sides; and thence was inferred the proportion of heat that passed off through the sides of the instrument, and what proportion must have escaped through its uncovered ends. With these data it is easy to infer, from an experiment with the ends covered, what proportion of the heat, lost in the cooling, had escaped through the flat terminal surfaces when covered. In this manner it has been ascertained that, assuming the total of the loss of heat emitted by a cylinder, in a given time, for *instance* $55\frac{1}{2}'$, to be 10,000, the quantity that escapes through the vertical sides will be =7,015, and that which penetrates through the terminal sides and coverings =2,985.

Admitting these computations, it will now appear how an estimate may be made, what proportion of the heat lost in any other experiment, actually escaped through the vertical sides of the instrument: and as the quantity of the heat emitted may well be represented by the time of the emission, there can be no difficulty in substituting the velocity for the quantity; whence it is inferred, that in the experiment, for instance, when the sides of one of the cylinders were blackened, the velocity with which heat is given off from the naked

sides of a cylinder, is to the velocity with which it is given off by the blackened sides, as 5,654 is to 10,000 very nearly, the velocities being as the times of cooling inversely.

Before he proceeds further in his investigation, the author finds it necessary to describe an additional instrument which he contrived for measuring, or rather for discovering, those very small changes of temperature in bodies which are occasioned by the radiations of other neighbouring bodies that happen to be at a higher or a lower temperature. This instrument, which he calls a *Thermoscope*, consists of two glass balls joined with and opening into the two ends of a glass tube, which is bent in two places at right angles, so that the balls, when the instrument is erected, are at the same horizontal height. A small quantity (about one drop) of coloured spirits of wine was introduced into this tube before it was finally closed, which, when the temperature of the air in the whole tube and the two balls is equal, keeps its place nearly at the middle of the lower or horizontal part of the tube. No sooner, however, does this perfect equilibrium cease, than the drop will move towards the side that is least heated. A scale is here applied, which indicates the difference of the temperature of the air in the two sides of the tube, and in the respective balls. A vertical screen between the two balls prevents the radiance of a heated body approached to one of them from affecting the other. This instrument was found of so delicate a sensibility, that the naked hand presented to one of the balls at the distance of several inches, would put the spirit of wine in motion, and the approach of a person at some feet from it would immediately affect it.

A conjecture is now proposed, which this instrument was intended to elucidate and probably confirm. There being great reason to conclude, that all the heat which a hot body loses when exposed to the air, is not given off to the air which comes into contact with it, but that a large proportion of it escapes in rays which do not heat the transparent medium through which they pass, but, like the rays of light, generate heat only then and there where they are intercepted and absorbed; it may hence be concluded, that in general, as has been in particular observed in the foregoing experiments, the cooling of the instruments is in fact promoted by the coverings applied to their surfaces; those coverings, considered as substances on which the rays impinge, being the means which in some way or other accelerate, or at least facilitate, the emission of calorific rays from the hot surfaces.

The first experiment, which has thrown some light upon this subject, was made with two brass cylinders equally heated, but in one of which one of the flat surfaces had been blackened, while the whole of the other cylinder was left in its polished state. The black surface of the one, and one of the bright surfaces of the other, were presented to the two opposite balls of the thermoscope, each to each, and at equal distances. Here the little column of spirit of wine in the tube beneath was instantly driven out of its place by the superior action of the blackened surface, and did not return to its former

station till the effect was compensated by proportionably altering the distances of the heated cylinders from the balls. In some further experiments, instead of blackening one of the flat surfaces of one of the cylinders, the other coverings used in the foregoing trials were applied, and the results were such as might have been expected. They all tended to prove that different bodies, or rather different surfaces, emit heat not by any conducting power in themselves, or in the surrounding bodies, but by a power which is here called *radiation*, the nature of which had hitherto escaped our notice.

Several experiments were next made with heated cylinders of different metals, but the results proved that all metals give off heat with the same facility, or rather with the same celerity. May not this, it is asked, be owing to their being all equally wanting in transparency? And does not this afford us a strong presumption that heat is in all cases excited and communicated by means of *radiations*, or as they may more properly be called *undulations*?

Before these questions can be solved, another and a very important point in this inquiry must be decided, viz. whether bodies are cooled in consequence of the rays they emit, or by those they receive? Our author was manifestly led to this problem by the celebrated experiment of Prof. Pictet, from which it appears that rays or emanations which (like light) may be concentrated by concave mirrors, proceed from cold bodies; and that these rays when so concentrated, are capable of affecting an air thermometer in a manner perfectly perceptible. The first experiment on this subject was to ascertain the existence of these cold emanations universally; and this being successfully effected, it is proved by other processes, the detail of which would far exceed our bounds, that the radiation of cold as well as of hot bodies being established, the rays which proceed from cold bodies have likewise the power of *generating cold* in warmer bodies which are exposed to their influence.

The object of another set of experiments was to ascertain whether all cold bodies at the same temperature emit the same quantity of rays; or whether (as is the case with respect to the calorific rays emitted by hot bodies,) some substances emit more of them than others. Here it was a great gratification to the author to find in the first experiment that the frigorific rays, from a blackened metallic surface, were much more powerful in generating cold than those which proceeded from a similar metallic surface of exactly the same temperature, but without any coating.

Observing that the approach of the hand to one of the balls of the thermoscope affected the indications very sensibly and rapidly, it occurred that perhaps animal substances emit both calorific and frigorific rays more copiously than other substances, and that probably living animal bodies emit them in still greater abundance than dead animal matter. This was confirmed by a very conclusive experiment, in which one of the metallic surfaces was covered with goldbeater's skin, and which surface emitted at least twenty-five times more calorific rays than a naked surface. The frigorific rays from the animal

substance were likewise found to be much more efficacious in producing cold than those from the polished surface; though in what proportion could not be ascertained with any degree of accuracy. In general, however, there is every reason to conclude that at equal *intervals of temperature*, the rays which generate cold are just as real and just as intense as those which generate heat, or that their actions are equally powerful in changing the temperature of neighbouring bodies.

Our author, ever doubtful of the existence of the *caloric* of the modern chemists, thinks himself authorized here to throw out the following observation respecting that favourite hypothesis. On a supposition that caloric has a real existence, and that heat or an increase of temperature in any body is caused by an *accumulation* of that substance in such body, the reflection of cold would indeed be impossible; and to maintain its reality must to all unprejudiced minds appear an absurdity.

By further experiments it is proved that all those circumstances which are favourable to the copious emission of calorific rays from the surfaces of hot bodies, are equally favourable to the copious emission of frigorific rays from such bodies when they are cold. That, on the other hand, those substances which part with heat with the greatest facility or celerity, are those which acquire it also most readily. Also that an animal substance, for instance goldbeater's skin, will throw off more heat, and be more sensibly affected by the frigorific rays of colder bodies when blackened, than when they are of their natural colour. This latter fact is applied as a proof of the great utility of the inhabitants of hot climates being of a black colour; and it is suggested that Europeans might find some relief by availing themselves of this circumstance when they visit the torrid zone. It is also surmised that the custom of savages inhabiting cold countries, of besmearing their bodies with oil or other unctuous matter, may have its utility by enabling their skins to reflect the parching frigorific rays that reach them from the atmosphere.

Another subject, which is here minutely investigated, is to ascertain what proportion of the heat emitted by a hot body is acquired or retained by the circumambient air; and the result yielded by several experiments and calculations turns out, rather unexpectedly, that this proportion is so little as $\frac{1}{77}$th of the whole. And it is also proved that a heated body, of a globular form, being suspended in the centre of another larger thin hollow sphere, at the same temperature as the air and the walls of the room, the vicinity of the two surfaces will sensibly retard the cooling of the hot body; and that if instead of one there be a number of thin concentric spheres of different diameters, the retardation of the cooling will be still greater. Combining with this the results of some former experiments, from which it appears that the cooling will be slower when the opposite surfaces are bright, than when they are unpolished or blackened, some inferences are derived concerning the warmth of different substances used as clothing, their effect in this respect, consistently with

the hypothesis of radiation, depending very much on the polish of their surfaces. Thus if those substances which supply the warmest coverings, such as furs, feathers, silk, &c. be viewed through a microscope, we shall find the surfaces of their fibres or minute laminæ not only smooth, but also very highly polished: and those substances will be warmest which excel in these respects, the fine white shining fur of a Russian hare being much warmer than coarse hair; and fine silk, as spun by the silk-worm, being preferable for warmth to the same silk twisted together into coarse threads.

A considerable part of the paper is now bestowed on the theory of heat, which the author attempts to deduce from the foregoing facts and observations. Heat and cold, he says, like fast and slow, are mere relative terms; and as there is no relation between motion and rest, so there can be none between any degree of heat and absolute cold, or a total privation of heat. It has long been thought, and it appears more and more probable, that *motion* is an essential quality inherent in all matter: this is illustrated by many examples; and by applying the analogy above given, and the observations since brought forward, there seems every reason to believe that, without having recourse to any specific element, all the phenomena of heat may be accounted for by the simple operations of *motion;* or that motion, in fact, constitutes the heat or temperature of sensible bodies.

It will no doubt occur that this theory will hardly account for the effects of frigorific rays; but this objection is answered by the observation, that as the rapid undulations occasioned in the surrounding ethereal fluid by the swift vibrations of a heated body will act as calorific rays on the neighbouring colder solid bodies; so the slower undulations occasioned by the vibrations of a cold body, will act as frigorific rays on neighbouring bodies of a higher temperature; and that these reciprocal actions will continue, but with decreasing intensity, till the two bodies have acquired the same degree of temperature, or until their vibrations have become isochronous.

According to this hypothesis, *cold* can with no more propriety be considered as the absence of *heat*, than a low or grave sound can be considered as the absence of a higher or more acute note; and the admission of rays which generate cold involves no absurdity, and creates no confusion of ideas.

As this theory, however, entirely supersedes the hypothesis of the calorific element, of late so much resorted to, it may be imagined that the author would not discuss the controversy in a slight or superficial manner; and accordingly many pages are here dedicated to this intricate and abstruse disquisition.

Among other important points, it was necessary to reconcile solidity, hardness, and elasticity, with the incessant motion he ascribes to the constituent particles of matter, and to obviate the objection founded on a supposition that there is not room sufficient for this motion. What increases the perplexity is, that, admitting the changes of temperature in bodies to be the effect of the calorific and frigorific radiations above described, a particular nicety will be required to

distinguish between the effects of those simultaneous operations, and of ascertaining their relative intensities. A hot body, A for instance, heats a neighbouring colder body B, by its calorific radiations; but B emits at the same time frigorific radiations, which contribute to lower the temperature of A; nor is it clear that both these bodies, especially if they have polished surfaces, will not reciprocally, and perhaps repeatedly, reflect those incident rays, and that those rays will not be refracted by the media through which they pass, and be concentrated or expanded by the shapes of the reflecting surfaces, and thus create a combination of effects, which it will require much labour and ingenuity to unravel.

As it is impossible for us within our narrow limits to do justice to the connected series of observations and arguments here adduced, we shall refer those who wish for more ample information on the subject to the paper itself; and this the rather, that we may dwell more largely upon the practical uses that may be derived from a knowledge of the facts which the author now considers as fully established.

In all cases where it is intended to preserve the heat of any substance which is confined in a metallic vessel, it will greatly contribute to that end if the external surface of that vessel be kept very clean and bright; but if the object be to cool anything quickly in such a vessel, its external surface should be painted, or coloured with some of those substances which have been found to emit calorific radiations in abundance. Hence the sides of kitchen utensils should be kept bright, in order to confine the heat; while their bottoms should be blackened, in order that their contents may be made to boil sooner, and with a less expense of fuel.

Brewers, it seems, are mistaken when they employ broad shallow vessels, or flats, as they call them, of metal for cooling their wort. Wooden flats, it appears, ought in every respect to have the preference.

In all cases when metallic tubes, filled with steam, are used for warming rooms or hot-houses, the external surface of those tubes should be painted, or covered with some substance which facilitates the emission of calorific rays. Where, on the other hand, tubes are intended to convey hot steam from one place to another, they should be kept very clean and bright. This applies also to the cylinders of steam-engines, and the principal tubes used in that machine.

Gardeners should advert to the circumstance, that if walls painted black acquire heat faster when exposed to the sun's direct rays, they likewise cool much faster during the night, and in the shade when the weather is cold.

Black cloths are known to be very warm in the sun; but they are far from being so in the shade, especially in cold weather, when the temperature of the air is below that of the surface of the skin.

It having been shown that the warmth of clothing depends much on the *polish* of the surface of the substance of which it is made, we may conclude that in choosing the colour of our winter garments, those dyes should be avoided which tend most to destroy that polish.

Hence there is reason to think that, contrary to the general opinion, white garments are warmer than any other in cold weather; and indeed if they are well calculated to reflect calorific rays in summer, they ought to be equally well calculated to reflect those frigorific rays by which we are annoyed in winter. Fur garments have been found by experience to be much warmer in cold weather, when worn with the hair outwards, than when it is turned inwards.

This is alleged as a proof that we are kept warm by our clothing not so much by confining the heat of our bodies, as by repelling those frigorific rays which tend to cool us. The fur of several delicate animals we know becomes white in winter in cold countries; and bears which inhabit the polar regions are likewise known to be white in all seasons. Now if, in fact, as there is great reason to believe, white is the colour most favourable to the reflection of calorific and frigorific rays, it must be acknowledged that these animals have been greatly favoured in having a clothing assigned them so well adapted to their local circumstances.

The excessive cold which is known to prevail, in all seasons, on the tops of high mountains, and the frosts at night which frequently take place on the surface of the plains below, seem to indicate that frigorific rays arrive continually at the surface of the earth from every part of the heavens; and it is no doubt by the action of these rays that our planet is continually cooled, and enabled to preserve the same mean temperature for ages, notwithstanding the immense quantities of heat that are generated at its surface by the continual action of the solar rays. The action of these frigorific nocturnal rays will likewise justify the inhabitants of hot climates, who, in order to be more cool during their hours of rest, remove their beds in summer to the tops of their houses.

Experiments and Observations on the Motion of the Sap in Trees. In a Letter from Thomas Andrew Knight, *Esq. to the Right Hon. Sir* Joseph Banks, *Bart. K.B. P.R.S.* Read February 16, 1804. [*Phil. Trans.* 1804, *p.* 183.]

Some experiments are here described, the tendency of which is to prove, what the author had advanced as a conjecture in a former communication, that the vessels of the bark which pass from the leaves to the roots, are in their organization better calculated to carry the fluids they contain towards the roots than in the opposite direction.

In the first of these experiments several strong horizontal shoots of vines were depressed about their middle; and at that part, buried in the mould, contained in pots about ten inches in diameter: after some months of vegetation, when the shoots had nearly filled the pots with roots, they were separated from the parent stock, having at each side above the earth a certain length of the layer, with at least one bud upon each. The end towards the stock was called the inverted, and the other the proper end of the layer. *If the author's*

above-mentioned conjecture of the retrograde motion of the sap be founded, it would follow that in the subsequent vegetation the inverted would display a more vigorous growth than the proper end; and this accordingly was soon found to be the case, with this additional circumstance, that the parts beyond the buds on the inverted ends were observed to increase considerably, while the same parts on the proper ends not only withered, but even gradually died away.

In another experiment a number of cuttings of gooseberry and currant trees were planted, some in their natural erect, and others in an inverted position. Many of these, especially the gooseberry cuttings, failed altogether; but in those that survived, the same accumulation of wood was observed on the upper ends of the inverted cuttings as on the vine shoots: similar effects were likewise observed in inverted grafts of the apple-tree, and in some respects also in cuttings from the sallow-tree, where, however, they being of some length, the accumulation of wood did not take place at the summit, but about the base of the cuttings.

It will be needless to dwell minutely on these results, since they may all be deduced from the author's theory, which, in addition to what has been above stated, is, nearly in his own words,—that the vessels of plants are not equally well calculated to carry their contents in opposite directions; and that the vessels of the bark, like those which constitute the venous system of animals (to which they are in many respects analogous), are provided with valves, imperceptible indeed to our eye on account of their extreme minuteness, but whose effects in directing the course of the sap are sufficiently obvious.

The paper concludes with some strictures on the experiments described by Hale and Du Hamel, and the reasons why these naturalists did not arrive at the same conclusions which are here brought forward, and an experiment which illustrates some parts of the paper the author gave last year on the descent of sap in trees.

Analytical Experiments and Observations on Lac. By Charles Hatchett, *Esq. F.R.S.* Read April 12, 1804. [*Phil. Trans.* 1804, *p.* 191.]

A brief historical account of the substance here treated of is prefixed to this paper. Though long in use, especially in India, yet, except what we have lately learnt from Mr. Kerr and Mr. Saunders, few inquiries have hitherto been made concerning its mode of production, first discovery, its nature and relative properties. We now know that it is the nidus or comb of the insect called Coccus, or Chermes Lacca, deposited on branches of certain species of Mimosa and other plants; and that the kingdom of Assam furnishes it in the greatest quantity. There are four sorts of it:—1. The stick lac, being the substance or comb in its natural state, incrusting small branches or twigs. 2. Seed lac, or the same substance granulated, but probably prepared in some manner, it being deprived of a great part of its colouring matter. 3. Lump lac, formed from seed lac,

liquefied by fire, and formed into cakes. And, 4. Shell lac, being the original comb, liquefied in water, strained through a cotton cloth, and spread upon a junk of a plantain-tree so as to form thin transparent laminæ: this kind contains the least of the tinging substance, as may well be expected from the mode in which it is prepared.

Among the chemists who have hitherto analysed this substance, none deserve notice except M. Geoffroy; but our author's present labours render his investigations of little or no avail.

The first section of this paper treats of the effects of different menstrua on the varieties of lac, from which it appears that it is soluble in alkalies, and in some of the acids. And the second section contains an account of the analytical experiments made on stick, seed, and shell lac. From the ample series of facts herein contained, of which it is in vain to attempt a compendious abstract, we collect in general that the varieties of lac consist of four ingredients, namely, extractive colouring matter, resin, gluten, and a peculiar kind of wax; and that the resin is the predominant ingredient, insomuch that, strictly speaking, we ought to consider lac as consisting principally of resin mixed with certain proportions of a particular kind of wax, gluten, and colouring extract. The mean results of the experiments give the proportions as follows:—100 parts of stick lac are found to contain resin 68, colouring extract 10, wax 6, gluten $5\frac{1}{2}$, and extraneous matter $6\frac{1}{2}$;—seed lac, resin $88\frac{1}{2}$, colouring extract $2\frac{1}{2}$, wax $4\frac{1}{2}$, gluten 2;—and shell lac, resin 90·90, colouring extract $\frac{1}{2}$, wax 4, and gluten 2·80. Each of these ingredients, we must observe, has been separately and carefully analysed.

The third and last section contains a number of general observations, chiefly relating to the uses of this substance. From the whole of the experiments here related, it appears that although lac be indisputably the production of insects, yet it possesses few of the characters of animal substances; and that the greater part of its aggregate properties, as well as those of its component ingredients, are such as more immediately appertain to vegetable bodies. Its uses are various, and some of them important. The Indians manufacture it into rings, beads, and other female ornaments. When formed into sealing-wax, it is employed as a japan, and is likewise manufactured into different coloured varnishes. The colouring part is formed into lakes for painters; and as a dyeing material it is in very general use. The resinous part is employed to make grindstones, by melting and mixing it with about three parts of sand, or with a like proportion of powder of corundum for those stones which are used by lapidaries. We owe to Mr. Wilkins the information, that a peculiar and excellent kind of ink is prepared by the Hindoos of shell lac, dissolved in water by the mere addition of a little borax, and by adding to the solution a certain quantity of ivory- or lamp-black. This process has the further advantage of teaching us to prepare an aqueous solution of lac, which probably will be found of very extensive utility, especially in the preparation of varnishes and pigments, which, when perfectly dry, will not be easily affected by damp or water.

The opinion generally adopted by chemists, that acids and alkalies do not act upon resinous bodies, appears from this investigation to be altogether erroneous; since the chief ingredient of lac which we have seen is soluble in those menstrua, is now determined to be of a resinous nature.

Some hints are lastly given concerning the further uses that may be made of these preparations in various manufactures, especially in dyeing, and the preparation of colours: nor is it thought unlikely that medicine may derive some advantages from the application of the extensive series of acid and alkaline solutions of resinous substances, which till now were thought to be unattainable.

On the Integration of certain differential Expressions, with which Problems in physical Astronomy are connected, &c. By Robert Woodhouse, *A.M. F.R.S. Fellow of Caius College.* Read April 12, 1804. [*Phil. Trans.* 1804, *p.* 219.]

In the preamble to this paper the author states, that if the introduction of the new calculi, as they have been called, has extended the bound of science, it has also greatly increased its difficulties by their number and magnitude: and that whilst the differential forms, which can be completely integrated, occur only in few problems, the investigations in physical astronomy give rise to differential expressions which call forth all the resources of the analytic art, even for their approximate integration.

The main object of this paper is to give a method of computing the integrals of certain expressions which lead to the determination of the logarithms of numbers, and the lengths of circular arcs. In treating of one of these expressions, known by the name of Fagnani's Theorem, the author traces out the correspondence between the methods of computation, and the proportion of geometrical figures; the analytical method, by which the integral expressing the arc of a circle is computed, affording, when duly translated, the theorem for the tangent of the sum of the two arcs expressed in terms of the tangents of the arcs.

It is in vain to attempt, without the use of symbols, to convey any adequate, nay, even a faint idea of the various series, converging and diverging according to the value of one of the coefficients of the original expression, which lead to the conclusions that illustrate this mode of investigation. Suffice it to say, that among other uses, the method may be applied to expand the formula that occurs in estimating the perturbation of planets: and in this instance the author points out the series which would be most commodious, and which would converge most rapidly if the radii of the orbits of the two planets, whose perturbations are sought, were nearly equal.

Observations on Basalt, and on the Transition from the vitreous to the stony Texture, which occurs in the gradual Refrigeration of melted Basalt; with some geological Remarks. In a Letter from Gregory Watt, *Esq. to the Right Hon.* Charles Greville, *V.P.R.S.* Read May 10, 1804. [*Phil. Trans.* 1804, *p.* 279.]

The number of conjectures that have been formed respecting the nature of basalt, and the variety of opinions hitherto entertained concerning its aqueous or volcanic origin, not having yet proved satisfactory to those who are more seriously bent upon the discovery of truth than the triumph of victory in a controversy; we are much indebted to the author of this paper for the ample body of observations and ingenious remarks it contains, from which, though they by no means establish either of the contested hypotheses, much may, however, be collected, which cannot fail to throw additional light on various intricate points in mineralogy and geology.

The main object here agitated, is the transition from the vitreous to the stony texture, which takes place in the refrigeration of glass; and the following experiment takes the lead in this investigation, and affords the materials for the subsequent disquisitions.

About 7 cwt. of the kind of basalt called Rowley Rag, of which a minute description is here given, were put into a common reverberatory furnace, strongly heated for several hours. It soon melted with a less degree of heat than would have fused an equal weight of pig iron, and subsided into the deeper part of the furnace in the form of a liquid but rather tenacious glass. A portion of it, on being taken out and suffered to cool, retained the character of perfect glass. But the remainder of the mass was left in the furnace, which was gradually cooled, and in eight days it was extracted, being cold on its surface, but still retaining a considerable degree of internal heat. Its shape being very irregular, it was so differently affected by the same degree of heat, and the same progressive refrigeration, that no uniformity of texture could be expected. This circumstance, however, which might have been obviated by care, is not to be regretted, since it has fortuitously disclosed the singular peculiarities in the arrangement of bodies passing from a vitreous to a stony state, which are the chief object of this paper.

A very minute description of this mass is next given, both in its progress towards vitrification, and in its subsequent refrigeration. What appears most relevant is, that in the fluid glass a tendency towards an arrangement of particles is first developed by the formation of minute globules, thickly disseminated throughout the mass. In the process of cooling, these globules adapt their form to their confined situation, gradually filling up every interstice; and they finally assume the appearance of a substance apparently homogeneous, equally unlike glass, and the parent basalt, but much resembling some varieties of jasper in the compactness of its texture, and in its opacity.

If the temperature adapted to the further arrangement of the

particles of the mass be continued, another change immediately commences, in the progress of which the whole acquires a more stony texture, and a greater degree of tenacity. This is generally effected by a gradual formation of secondary spheroids in the heart of the jaspideous substance, whose centres are more distant, and dimensions greater than those of the above-mentioned globules: they are radiated with distinct fibres. When two of these spheroids come into contact by mutual enlargement, no intermixture of their fibres seems to take place; they reciprocally compress each other, and their limits are defined by planes, at which a distinct separation takes place. When several spheroids come in contact on the same level, they are formed by mutual pressure into prisms of tolerable regularity, whose division is perfectly defined: and when a spheroid is surrounded on all sides by others, it is compressed into an irregular polyhedron.

The transition from this fibrous state to a different arrangement, seems to be very rapid, for the centres of most of the spheroids become quite compact before they receive one quarter of their usual dimensions; the mass then becomes perfectly solid, very tenacious, and opake; and its hardness is somewhat inferior to that of the glass from which it is formed.

A further continuation of the temperature, favourable to arrangement, speedily occasions another change. The texture of the mass becomes more granular, and the brilliant points it exhibited in its former state become larger and more numerous, arrange themselves into regular forms, and finally, the whole mass becomes pervaded by thin crystalline laminæ, which intersect it in every direction, and form projecting crystals in the cavities.

It is thought that an equalized temperature would have rendered the whole mass at once similar to the substance last described; but then the interesting initial phenomena, from which the important inferences here announced are deduced, would not have been discovered.

These, and many more facts relating to the experiment, having been minutely detailed, the author proceeds to offer what he deems a partial explanation of the formation of the globules and of the radiated spheroids. It is well ascertained, he says, that heat is emitted by all bodies in their change from a gaseous to a fluid state; and it is reasonable to suppose that heat may also be emitted in those changes of arrangement which affect the internal texture of a body after it has attained an apparently solid state.

That a succession of such changes actually takes place, seems to be demonstrated by several of the appearances in the experiment, and particularly by the increase of specific gravity, which generally keeps pace with the internal changes of the substance. These changes, it is conjectured, may be caused by a gradual diminution of temperature, which permits certain laws to induce peculiar arrangements among the particles of the glass: when several of these particles enter into this new bond of association, they must form a minute point, from which heat will issue in every direction: that heat will

gradually propagate itself till the temperature of the glass is equalized, and then the recurrence of the circumstances which induced the first particles to arrange themselves will cause other particles to arrange themselves also; and these the attraction of aggregation will dispose round the point first formed. A second emission of heat in every direction will now take place; the temperature will again be equalized; and again another concentric coat of arranged particles will apply itself to the little globule. That these globules are formed of concentric coats does not clash with the circumstance of their being likewise radiated; as every one may have remarked the connexion that almost uniformly exists between the radiated structure and the formation by concentric coats; the more obvious instances of which are the hæmatites and the calcareous stalactites. In what manner this is likely to be effected is stated at some length in the paper.

Some curious remarks are next made on the observation of Mr. Smithson, that solution, far from being necessary to crystallization, effectually prevents its commencement; since, while solution subsists, crystallization cannot take place: and many of the phenomena being duly considered, it seems most probable that the particles of bodies apparently solid must be capable of some internal motion, enabling them to arrange themselves according to their crystalline polarity while they are in a solid state. Among the instances given, are the conversion of glass vessels into Reaumur's porcelain, the tempering of steel, and the process of annealing. This does not altogether disprove the crystallizations formed by molecules suspended in aqueous solutions; but it is in general insisted upon, that all crystallizations are dependent on heat, there being, in fact, no fluidity, and consequently no solution, which heat does not produce.

These observations tend to prove the analogy which exists between the igneous and aqueous formations, and to show that precisely the same order and kind of arrangement is followed in the generation of stony masses from water as from fire. Among the many instances that are adduced to justify this assertion, are, on the one hand, the phenomena exhibited by lavas, in which may be observed every step of the passage from the vitreous to the stony, from that to a porphyritic, and finally to the granitic state: on the other hand, we may select the formation of calcareous stalactites, in which the successive depositions of calcareous carbonate form a mass which at first is fibrous; a continuance of the process causes the fibrous structure to disappear, and the stalactite becomes irregularly spathose; after which the irregularities vanish altogether, and it becomes perfect calcareous spar, divisible into large rhomboids, with the form peculiar to that mineral.

Adverting now to the chief object of this paper,—the basalt,—the author observes, that should, in fact, the analogy between the aqueous and igneous formation appear founded, the transition from glass to stone can no way affect the great question which has so long divided geologists about the origin of basalt: for though it be synthetically demonstrated that basalt may be formed by fire, the proofs in favour

of its formation by water must be allowed to be at least of equal weight. In fact, while the frequent instances of petrifactions found in basalt support the aqueous hypothesis, the equally numerous indications that the heat emanating from it has manifestly changed beds of coal into coke and indurated strata of stony substances, strongly argue in favour of the igneous assumption.

The above sketch of the author's observations and reasonings, imperfect as it is, may however suffice for rightly comprehending the interesting part of the paper which relates to the wonderful regularity of the prismatic configuration of basaltic columns, and also for their articulations. If we suppose that a mass of fluid basalt has filled a valley to an indefinite depth and extent, the process of arrangement in its particles must be deduced from the removal of its heat or moisture, according as its solution is igneous or aqueous. This can only be done by the action of the atmosphere on its upper surface, and by the ground on which it reposes absorbing the heat or moisture from its under surface.

From the variations of the atmosphere, its action must be irregular; and from the perpetual change of the parts in contact with the heated or moist surface, its operations will always be nearly as active as at first, allowance being made for its variations: but the absorption of the ground will be regular, and regularly diminishing in activity, in proportion as the parts near the mass approach nearer to the same temperature or same moisture with the mass above; and thus absorption can only be carried on by the transmission of heat or moisture from the mass to the solid rocks below.

From these considerations, it seems evident that the arrangement of the part of the basaltic mass near the ground will be begun with more energy than it can be continued, and that the results will be more slow and regular, and that induced by the action of the atmosphere. After the first stage in the process of arrangement has been performed, and a stratum of the jaspideous substance is extended over the surface of the ground, there seems no reason to doubt that a number of radiated spheroids will be generated in it, having probably all their centres nearly at the same distance from the ground; and as the arranging power undergoes a gradual diminution of energy, it is not likely that two rows of them in height should be formed at once. In a word, it seems most probable, that in the arrangement of a mass of basalt, a single layer of radiated spheroids will be formed, reposing on the ground which supports the mass.

How these radiated spheroids, by coming in close contact, will compress each other, and form polyhedral and, generally, hexagonal prisms, will be understood from what we have said above of that operation in the experiment. If these prisms are resisted below, and there is no opposing cause above them, it is clear that they will extend their dimensions upwards into the undisturbed central mass of the fluid, till their structure is deranged by the action of the atmosphere on the upper surface of the basalt. According to this arrangement, the same cause that determines the concentric fractures of the

fibres of the spheroids, will produce convex articulations in the lower joints of the prisms. If the generating centres are not equidistant, the forms of the pillars will be irregular, and of different number of angles; and as the compression of the fibres will be greatest on the level of the generating centres, the lower part of the prisms will be most compact.

All the observations hitherto made on the great basaltic masses in nature, seem to confirm this simple theory; and the author bestows some pains in accounting for the appearances which seem at first sight not to agree perfectly with it. Those who shall peruse the paper will, if we are not much mistaken, be particularly gratified with the variety of information they will meet with in this part of the treatise.

Lastly, the author directs his attention to the many instances of other substances, besides basalt, which affect a columnar form, and which afford convincing proofs that their configuration is not confined to either the aqueous or igneous formation. Such are, certain lavas, columns of porphyry found near Dresden, a bed of gypsum at Montmartre, and other masses of various nature. Sandstone, clay, argillaceous iron ore, and many other substances, become prismatic by torrefaction; and prisms of starch formed in drying have often been considered as illustrative of basaltic formation. Some of these are probably to be attributed solely to contraction; and it is shown that they do by no means contribute toward any explanation of the process here in contemplation.

An Analysis of the magnetical Pyrites; with Remarks on some of the other Sulphurets of Iron. By Charles Hatchett, *Esq. F.R.S.* Read May 17, 1804. [*Phil. Trans.* 1804, *p.* 315.]

The substance which is the subject of this paper was, till lately, found only in some parts of Norway and Germany; but it now appears, from some specimens in Mr. Greville's collection, that it is likewise to be met with in considerable quantities near the foot of Snowdon in Caernarvonshire. The character by which this kind of sulphuret is chiefly distinguished from the other martial pyrites, is its magnetic property, by which, especially if it have been placed some time between magnetical bars, it will turn a needle completely round, attract and take up abundance of iron filings, and retain this addition to its original power for a considerable length of time.

After an accurate description of the external characters of this ore, Mr. Hatchett enters into a full account of the processes he instituted in order to discover its nature and component parts. In hopes of discovering the cause of the magnetic property which is peculiar to this species, he has entered into an analysis of the other kinds of martial sulphurets, not only natural, but also artificial, and has also paid particular attention to the experiments of others on this subject, particularly those of Mr. Proust, the learned Professor of Chemistry at Madrid, who has taken considerable pains in ascertain-

ing the constituent parts of this ore, both analytically and synthetically. In the progress of this inquiry, a certain analogy soon presented itself, between these compounds of sulphur and iron and those of other inflammable substances, such as carbon and phosphorus, with the same metal, which suggested the idea of some experiments on the subject; from whence various facts were deduced, not only of much curiosity, but also likely to lead to some useful purposes, as will appear hereafter. The principal results of this laborious investigation are,—

1. That the component ingredients of the magnetical pyrites are sulphur, and iron in the metallic state, the former being to the latter in the proportion nearly of 37 to 63.

2. That the chemical and other properties of this substance are very different from those of the common martial pyrites; which, however, are likewise composed of sulphur and iron, but varying in proportion from between 52 to 54 of sulphur, and 48 to 46 of metallic iron. Whence it appears, that the relative proportions in the composition of the magnetical pyrites, and of the mean of the common pyrites, is between 16 and 17.

3. That as the magnetical pyrites agrees in analytical results, as well as in all chemical and other properties, with that sulphuret of iron which hitherto has been only known as an artificial product, there is no doubt that it is identically the same, and that its proportions are probably subjected to a certain law, which, under certain circumstances, may be supposed to act in an almost invariable manner.

4. That in the formation of the common martial pyrites, there is a deviation from this law, and that sulphur becomes a predominant ingredient, variable in quantity, but which, by the present experiments, has not been found to exceed between 54 and 55 per cent.; a proportion, however, which possibly may be surpassed in other pyrites which have not as yet been chemically examined.

5. That iron, when combined naturally or artificially with 36 or 37 per cent. of sulphur, is not only still capable of receiving the magnetic fluid, but it is also rendered capable of retaining it, so as to become in every respect a complete magnet. And the same may in a great measure be inferred respecting iron which has been artificially combined with 45½ per cent. of sulphur.

6. That beyond this proportion of 45½ per cent. of sulphur, (at least in the natural common pyrites,) all susceptibility in iron of the magnetic influence appears to be destroyed; and although the precise maximum, which is capable of producing this effect, has not as yet been determined by actual experiment, it is certain that the limits are between 45 and 52; unless some alteration has taken place in the state of the sulphur or iron in the common martial pyrites, different from that which is conceived according to the present state of chemical knowledge.

7. That as carbon, when combined in a certain proportion with iron (forming steel), enables it to become a permanent magnet, and

as a certain proportion of sulphur communicates the same quality to iron, so are the effects of phosphorus found to be; phosphoret of iron being, in this respect, much the most powerful, at least when considered comparatively with sulphuret of iron.

8; and lastly. That as carbon, sulphur, and phosphorus, produce, by their union with iron, many chemical effects, of much similarity, so do each of them, when combined with that metal in certain proportions, not only permit it to receive, but also give it the peculiar power of retaining the magnetical properties; and thus henceforth, in addition to that carburet of iron called steel, certain sulphurets and phosphurets of iron may be regarded as bodies peculiarly susceptible of strong magnetical impregnation.

Among the observations which are subjoined to this paper, we find some remarks on the vitriolization of pyrites; from which we collect, that, contrary to the opinion of Mr. Proust, who thought that only those pyrites in which the proportion of sulphur is very small are liable to this change, the vitriolization is not so much owing to the proportion as to the state of the sulphur in the compound; and that this state is probably the effect of a small portion of oxygen, previously combined with a part or with the general mass of the sulphur at the time of the original formation of the substances; so that the state of this ingredient is tending to that of oxide.

It is, no doubt, remarkable, that the magnetical properties of the sulphuret of iron, which forms the principal subject of this paper, should never have been adverted to by any of the writers on magnetism. The few who observed it in the natural magnetical pyrites chose to ascribe it to particles of common magnetical iron interspersed in the ore: but from what has been stated, it is evident that this opinion must be relinquished; since there are certain known proportions of sulphur, as well as of carbon and phosphorus, beyond which the magnetical property will not be obtained, though the proportions beyond this maximum would by no means exclude the interposition of particles of iron. How far the combinations of magnetical sulphurets, carburets, and phosphorets may contribute towards the making artificial loadstones of greater strength than those hitherto known, is a subject recommended to the attention of future observers.

Remarks on the voluntary Expansion of the Skin of the Neck, in the Cobra de Capello or hooded Snake of the East Indies. By Patrick Russell, *M.D. F.R.S. With a Description of the Structure of the Parts which perform that Office. By* Everard Home, *Esq. F.R.S.* Read June 14, 1804. [*Phil. Trans.* 1804, *p.* 346.]

The information we gather from this paper is, that the remarkable expansion of the skin of the neck, which constitutes a principal character in this species, is a voluntary action, distinct from that inflation which all serpents, when irritated, are more or less capable of: that it is owing to a particular set of ribs situated at the neck of the

animal, and hence called cervical ribs. These ribs are about twenty-five in number, and gradually lengthen from the upper end to the tenth or eleventh pair, and then successively shorten to the last. They extend in lateral directions, having only a slight curvature; and when depressed, lie upon the side of the spine, one on the other. They are raised by four sets of muscles; and another large set of very long muscles has the power of bringing the skin forward, thus forming the appearance which has been called a hood. Besides these muscles, there are three other sets, by which the hood is depressed, and the parts are restored to that state in which the neck of the animal does not appear disproportionally protuberant. These descriptions are illustrated by accurate drawings; but no conjecture is here given as to the probable uses of this singular mechanism, except that it does not appear to promote in any way the play of the lungs, but that the expansion it produces may perhaps facilitate a dilatation of the gullet, for the purpose of allowing the snake to swallow its prey more easily.

Continuation of an Accourt of the Changes that have happened in the relative Situation of double Stars. By William Herschel, *LL.D. F.R.S.* Read June 7, 1804. [*Phil. Trans.* 1804, *p.* 353.]

In the former part of this paper, Dr. Herschel mentioned the changes he had noticed in the situation of six double stars; and in investigating the causes of those changes, he declared that he had recourse to the most authentic observations he could find of their motions in right ascensions and polar distance, especially in the instance of the double star Castor: but finding in the tables which have been lately published in the last volume of the Greenwich Observations, which give the proper motions of thirty-six stars, that (especially in the instance of the above-named star,) the motions are somewhat different from those he assigned to them in his former communication, he now undertakes to review the arguments he there used, in order to ascertain what will be the result of these new motions. As this investigation, which forms the first part of the present paper, has a continual reference to the contents of the preceding one, it will be in vain to attempt an abridgement, which could not be rendered intelligible within our usual limits. Nor can we enter here into a detail of the sequel of Dr. Herschel's observations on the changes in the situation of a great number of additional double stars; this second part of the paper, in which they are fully detailed, being itself a minute of his proceedings, in which he is at particular pains to point out that these changes of situation are not the effect of parallax.

Observations on the Change of some of the proximate Principles of Vegetables into Bitumen; with analytical Experiments on a peculiar Substance which is found with the Bovey Coal. By Charles Hatchett, *Esq. F.R.S.* Read June 14, 1804. [*Phil. Trans.* 1804, *p.* 385.]

Among the several spontaneous permutations in the productions of nature, none perhaps are more striking, and in many cases more unaccountable, than those which transfer bodies from one kingdom of nature into another: and those changes which transform organized into fossil substances are certainly not the least extraordinary and instructive.

The most numerous instances of this transformation are, no doubt, what we distinguish by the name of *Extraneous Fossils*; some of which still retain part of their original substance, whilst others can only be regarded as casts or impressions. An attentive observer will soon perceive a kind of gradation in these fossils, whether from animals or vegetables, commencing with those whose matter retains a marked analogy with that of the recent substances, and terminating in bodies decidedly mineral. And a curious remark occurs here,—that as animal petrifactions are most commonly of a calcareous nature, so, on the contrary, vegetable petrifactions are generally siliceous.

Without entering any further into a general disquisition on this important subject, our author proposes to discuss, in this paper, one particular case of the changes which organized, and especially vegetable, substances undergo, by being long buried in earthy strata, and thus exposed to the effects of mineral agents: and the instance he selects is the bituminous substances, concerning which he has long suspected that they are derived from the organized kingdoms, and especially from the resin and juices of vegetable substances, by the action of some of the mineral principles.

He cites three instances in this kingdom in which nature points out these changes, and which exhibit the gradations above intimated. These are,—1. The submarine forest at Sutton, on the coast of Lincolnshire, the timber of which has not suffered any very apparent change in its vegetable characters; 2. The strata of bituminous wood (called Bovey Coal) found at Bovey, in Devonshire, which exhibit a series of gradations, from the most perfect ligneous texture to a substance nearly approaching to the characters of pit-coal; and 3. All the varieties of pit-coal, so abundant in many parts of this country, in which almost every appearance of vegetable origin has been obliterated.

As the Bovey coal appears to be the mean in that gradation, and therefore most likely to afford instructive results, our author resolved to limit his inquiry into this process of nature, which may not improperly be called Carbonization, to that fossil, and to a peculiar bituminous substance with which it is often accompanied. But here he finds it expedient to premise some observations on a remarkable

schistus found at Reykum, one of the great spouting hot springs in Iceland. The singularity of this substance is, that a great part of it consists of leaves, (evidently those of the alder,) interposed between the different lamellæ. These leaves appeared to be in the state of charcoal; but on more close examination, no doubt remained of their still retaining a certain portion of some of the other principles of the original vegetable, such as extract and resin. This, in fact, is the result of an extensive chemical process, from which we learn that the schistus, taken collectively, yields, besides silicia, alumina, and oxide of iron, a certain proportion of water and of vegetable matter, and that it evidently belongs to the family of argillaceous schistus.

The above process may be considered as preliminary to that on the Bovey coal, in which the vegetable characters are more obliterated than in the leaves of the schistus. This coal, we are told, bears a great resemblance to a fossil found in Iceland, called Surturbrand; the strata of both being composed of trunks of trees, which have completely lost their cylindrical form, and are flattened, as if they had been subjected to an immense degree of pressure. On inquiring into this last-mentioned circumstance, our author produces his reasons for believing that it is not the effect of the mere pressure of a superincumbent stratum, but also of a certain change in the solidity of the vegetable bodies, and a powerful mechanical action, produced by the contraction of the argillaceous strata in consequence of desiccation.

Here follows the analysis of the Bovey coal. The results point out a great resemblance between this substance and that which forms the leaves contained in the Iceland schistus. The only exception is, that the leaves contain some vegetable extract, none of which could be discovered in the coal. Both consist of woody fibre in a state of semicarbonization, impregnated with bitumen and a small portion of resin, perfectly similar to that which is contained in many recent vegetable characters, and is but partially and imperfectly converted into coal; so, in like manner, some of the other vegetable principles have only suffered a partial change. Next to this woody fibre, resin is thought to be the substance which, in vegetables passing to the fossil state, most powerfully resists any alteration, and which, when this change is at length effected, is more immediately the substance from which bitumen is produced.

This opinion, that the vegetable extract and resin are the parts of the original vegetables, which retain their nature after other portions of the same have been modified into bitumen, is corroborated by the analysis which here follows, of a singular substance which is found with the Bovey coal. Dr. Milles, who first mentioned this substance, considered it as a loam saturated with petroleum; but our author, on mere inspection, decided that it is not a loam, but a peculiar bituminous substance. After a description of its external appearances, and some of its relative properties, we come to the analysis; from which we collect, that this is a peculiar and hitherto unknown substance, which is partly in the state of vegetable resin, and partly in

that of the bitumen called Asphaltum; the resin being in the largest proportion, 100 grains affording 55 of resin, and 44 of asphaltum. Thus we have an instance of a substance found under circumstances which constitute a fossil, although the character of it partly appertain to the vegetable and partly to the mineral kingdom

In the concluding section the author enters into an inquiry on the action of alcohol on resins and bitumens. Its power of dissolving the former is well known; but, contrary to the adopted opinion, he found that it also acted on bitumen, though indeed in a slight degree. His chief object was to ascertain whether a small portion of resin is contained in any of the bitumens, or, if not, to determine the nature of the substance which can be separated, although very sparingly, from those substances by digestion in alcohol. The results prove that the small portion which can be extracted from bitumen by digestion with alcohol is petroleum.

From a general view of the subject, the author thinks himself justified in asserting, that in bitumens the process of transformation appears to have been completed; whereas in the Bovey coal, and especially in the substance which accompanies it, Nature seems to have performed only half of her work, and, from some unknown cause, to have stopped in the middle of her operations. By this circumstance, however, much light is thrown on the history of bituminous substances; and the opinion that they owe their origin to the organized kingdoms of nature, and especially to the vegetable, which hitherto had been supported only by presumptive proofs, seems now to receive a full confirmation, although the causes which operate these changes on vegetable bodies are as yet undiscovered.

On two Metals, found in the black Powder remaining after the Solution of Platina. By Smithson Tennant, *Esq. F.R.S.* Read June 21, 1804. [*Phil. Trans.* 1804, *p.* 411.]

From a few experiments the author made in the course of last summer on this powder, he concluded, that it does not, as was generally believed, consist chiefly of plumbago, but that it contains also some other unknown metallic ingredient. Since those experiments, two French chemists, Messrs. Descotils and Vauquelin, having likewise examined that substance, found the same new metal; but neither of them observed that it contains moreover another metal different from any hitherto known.

The black powder used in the process, which is the subject of this paper, was obtained from platina carefully separated from all extraneous particles; so that the above ingredients, if found, must have been contained in that metal.

The first set of experiments relates to the effects produced by this powder when alloyed with other metals. It combines readily with lead; but the compound, even when the lead greatly predominates, is not very fusible. With bismuth, zinc, and tin, the effects are nearly similar; but with copper, a strong heat produces a much more

intimate union. The union of this substance with silver and gold produced upon it very little alteration; but, what is most remarkable, it could not be separated from these metals by the usual processes of refining. The alloys retain a considerable share of ductility; and the colour of that which is alloyed with gold is not materially different from that of pure gold.

The next experiments relate to the analysis of the black powder, and the properties of the two metals which enter into its composition. The method of dissolving the powder was similar to that employed by M. Vauquelin, viz. by the alternate action of caustic alkali and of an acid. The acid solution was found to contain that particular metal which has been noticed by Descotils. And of this metal, a considerable number of characters are here described, in addition to those already mentioned by the French chemists.

As to the alkaline solution, which Vauquelin considered as containing a portion of oxide of chrome, it is observed, that though some kinds of platina may contain chrome, and of course exhibit a certain quantity of it in its black powder, yet the precipitate which, upon accurate investigation, is yielded by it affords a very volatile metallic oxide, which evidently has not the characteristic qualities of that metal. As it is expedient to assign a specific name to every new substance, our author wishes to distinguish this precipitate by the appellation of Osmium, from the strong smell it emits. After showing in what manner it may be expelled from the alkali by an acid, and obtained by solution with water and distillation, the author mentions many of its relative properties and characteristic qualities. The most striking test of this oxide, we are told, is the mixture of its solution with an infusion of galls, which presently produces a purple colour, and becomes soon after of a deep vivid blue. It parts freely with its oxygen to all metals excepting gold and platina.

On a new Metal, found in crude Platina. By William Hyde Wollaston, *M.D. F.R.S.* Read June 21, 1804. [*Phil. Trans.* 1804, *p.* 419.]

Dr. Wollaston having conceived an idea that, in addition to the two new metals the preceding paper states to have been discovered in platina, the fluid which remains after the precipitation of that metal by sal-ammoniac, and which is likely to contain the more soluble parts of the mineral, might, on further examination, be found to contain some other substance worthy of our attention; and, in fact, having instituted an accurate analysis, of which the present paper contains a full detail, he thinks he has proved the existence of another unknown metal, to which, for the sake of distinction, he ascribes the name of Rhodium, from the beautiful rose-colour of the salts containing it. In the course of his detail, he likewise states the results of various experiments, which, he says, have convinced him that the metallic substance which was last year announced to the public by the name of Palladium, is contained (though in very small proportion,) in the ore of platina.

The process referred to for separating these several ingredients from each other yielded, in fact, a pure metallic button, not malleable, but uniting readily with all the other metals that have been tried, except mercury, and whose specific gravity appeared not less than 11. This is the rhodium, which is here announced for the first time.

The palladium was precipitated from the alcohol employed for washing the salt of rhodium: it was yielded, indeed, in a very small proportion, but in sufficient quantity, however, to prove that it is actually a simple metal residing in platina, and to induce a suspicion of some error in Mr. Chenevix's investigation, who thought it a compound of platina and mercury; but our author candidly adds, that he has made several attempts to imitate the synthetical experiments of that chemist by solution and amalgamation, but without success.

The Croonian *Lecture on Muscular Motion. By* Anthony Carlisle, *Esq. F.R.S.* Read November 8, 1804. [*Phil. Trans.* 1805, *p.* 1.]

Admitting that there are subjects in the economy of nature which will ever elude our most attentive observation, and that many institutions similar to our Croonian Lecture will probably never attain the end for which they were founded, it cannot, however, be denied that several of them, and ours in particular, have at different times brought forward various collateral, and some of them not unimportant facts, which have in some measure contributed to extend our knowledge of nature. This latter is the point of view in which the present communication is to be considered; concerning which the author says, that, waving the investigation of the general theory of muscular motion, he shall limit his present inquiry to certain circumstances which are connected with this motion, considered as causes, or rather as a series of events, all of which contribute more or less as essential requisites to the phenomena. The changes which obtain in muscles during their contractions or relaxations, and their corresponding connexions with the vascular, respiratory, and nervous systems, are, he declares, the chief objects of his present investigation.

The lecture is divided into six sections, of which the following are the heads, together with some of the most prominent facts contained under each of them; the nature of the performance, which consists chiefly of insulated facts, and our limits in point of time, precluding us from being so minute in our analysis as the importance of the subject may be thought to require.

Sect. 1. *Of the physical and sensible properties of muscles, considered as distinct parts of an animal, and as peculiar organs.*—In describing the fasciculated texture of the fibres which compose a muscle, and the elasticity of these fibres during the contracted state of the muscle, the author advances an opinion, that this elasticity appears to belong to the enveloping reticular or cellular membrane, and that it may be safely assumed that the intrinsic matter of muscle is not elastic.

The attraction of cohesion in the parts of muscle appears to be strongest in the direction of the fibres, and to be double that of the contrary or transverse direction. When muscles cease to be irritable, this attraction in the direction of the fibres is diminished; but it remains unaltered in the transverse direction.

When muscles act more powerfully or more rapidly than is proportionate to the strength of the sustaining parts, they do not usually rupture their fleshy fibres, but generally break their tendons, or even an intervening bone; whence it is inferred, that the attraction of *cohesion is more active and powerful in the contracted state of the* muscle than during its relaxed or passive state.

The muscular parts of different classes of animals vary materially in colour and texture; and such variations occur not unfrequently in different parts of the same individual.

Sect. 2. *Of the anatomical structure of muscles, and their relations with other parts of the animal body.*—The lecturer in this section professes to give no more than a rapid sketch of the history of muscular structure. One example of the origin of a muscle he deduces from the process of the incubated egg: but here it remains doubtful whether the rudiments of the punctum saliens be part of the cicatricula organized by the parent, or merely a structure resulting from the first process of incubation. The anatomical structure of muscular fibres, he next observes, is generally complex, according as they are connected with membrane, blood-vessels, nerves, and lymphæducts; which seem to be only appendages of convenience to the essential matter of muscle.

A muscular fibre, being carefully inspected in a powerful microscope, is found to be a solid cylinder, the covering of which, as had already been intimated in a previous part of the lecture, is a reticular membrane, and the contained part a pulpy substance, irregularly granulated, and of scarce any cohesive power when dead.

The arteries articulate copiously upon the reticular coat of the muscular fibre; they anastomose with corresponding veins; but this continued canal is not supposed to act in a direct manner upon the matter of muscle. In what manner the capillary arteries terminating in the muscular fibre may effect all the changes of increase in the bulk or number of fibres, in the replenishment of exhausted materials, and in the repair of injuries, is as yet matter of conjecture; but these arteries, it is thought, must be secretory vessels for depositing the muscular matter, the lymphæducts serving to remove the superfluous fluids and the decayed substances which are unfit for use. These lymphæducts appear to receive the fluids they contain, not, as has been represented, from the projecting open ends of tubes, but from the interstitial spaces formed by the reticular or cellular membrane.

The functions of nerves in the muscular system are the next object of contemplation. These also, it seems, terminate in the reticular or cellular membrane, the common integument, and the connecting medium of all the dissimilar parts of an animal. We have to regret

that we cannot dwell longer on this curious part of the lecture, especially where it treats of the combination, by means of nerves between animated and what may be deemed inanimate matter, as in the instances of bones, shells, teeth, and other extravascular and insensible substances, which, when completed, are no longer alterable by the animal functions.

Sect. 3. *Of the connexions between the functions of muscles and the temperature and respiration of the animal.*—That different parts of an animal are susceptible of different degrees of temperature, is a fact which stands in no need of demonstration; and it is equally obvious that every animal, besides being susceptible of the external changes of temperature, possesses also the power of generating heat within itself. This last-mentioned power is ascribed, in a great measure, to respiration; and this opens a field of inquiry, in what manner and to what degree the different classes of animals are possessed of that faculty. In this disquisition we find, among other interesting facts, a curious account in what manner hybernating animals are enabled to subsist several months without respiration.

The irritability of the heart, we are next told, is inseparably connected with respiration, and the blood appears to be the medium of conveying heat to the different parts of the body; and hence it is naturally inferred, that the changes of animal temperature in the same individual are always connected with, and proportionate to, the velocity of the circulation. After death, the blood of an animal is presently coagulated, and the muscles are usually contracted; but, from some observations here stated, it appears that the final contraction of muscles is not inseparable from coagulation of the blood within them, nor of a change in the reticular membrane. Lastly, it is asserted, that the reiterated influx of blood is not essential to muscular irritability; since the limbs of animals, separated from the body, continue for a long time afterwards capable of contractions and relaxations.

Sect. 4. *Of the application of chemistry to this subject.*—Our lecturer asserts, under this head, that the constituent elements of both animal and vegetable substances are not separable by any chemical process hitherto instituted, in such a manner as to admit of a synthetical re-combination: and he maintains, that, until such a recombination can be effected, all chemical discussions and investigations on the matter of muscles are not likely to afford any conclusive illustration.

Sect. 5. *Facts and experiments tending to support and illustrate the preceding arguments.*—We meet here with a number of observations on the heat of the blood and viscera of animals of different classes; also on the effects of crimping fish, which produces not only a sensible rigidity or contraction, but also an increase of specific gravity, in the muscles. Muscular fibres of quadrupeds, being immersed in water of a low temperature, gave also manifest proofs of contractions occasioned thereby. In the heat of 100°, the muscles of cold-blooded animals, and at 110° those of the warm-blooded, fall into the con-

tractions of death. The latter always contain more red particles in their substance than those of cold blood, and are sooner deprived of their irritability, even though their relative temperature be preserved. It appears, also, that respiration in the former tribe is more essential to life than in the latter.

Various experiments are next mentioned on the substances which accelerate the cessation of irritability in muscles when applied to their naked fibrils, such as all narcotic vegetables, poisons, muriate of soda, the bile of animals, &c. Discharges of electricity, passed through muscles, destroy their irritability, but leave them apparently inflated with small bubbles of gas, owing, perhaps, to some combination which decomposes water. Workmen who are exposed to the contact of white lead, nitric acid, or quicksilver, frequently experience local spasms or partial palsy.

Lastly, some arguments are adduced which prove that a smaller quantity of blood flows through a muscle in the state of contraction than during its quiescent state; that when muscles are vigorously contracted, their sensibility to pain is nearly destroyed; and that the human muscles are susceptible of considerable changes, from extraordinary impressions on the mind, such as grief, fear, uncommon attention, mental derangement, &c.; in all which cases uncommon muscular exertions have been observed, which could not have been affected without the operation of those stimulants.

Sect. 6.—This section contains some conclusive remarks, chiefly on the effects of stimuli on the muscles, as they are distinguished into voluntary, involuntary, and mixed. For the classification of these agents here stated, we must refer the curious physiologist to the paper itself; having already, perhaps, trespassed too far upon the time that can well be spared for the abstract of this lecture.

Experiments for ascertaining how far Telescopes will enable us to determine very small Angles, and to distinguish the real from the spurious Diameters of celestial and terrestrial Objects: with an Application of the Result of these Experiments to a Series of Observations on the Nature and Magnitude of Mr. Harding's *lately discovered Star. By* William Herschel, *LL.D. F.R.S.* Read December 6, 1804. [*Phil. Trans.* 1805, *p.* 31.]

Dr. Herschel commences his paper by stating, that, being desirous of ascertaining the magnitude of the moving celestial body lately discovered by Mr. Harding, and intending, for that purpose, to make use of a ten-feet reflector, it appeared to him a desideratum highly worthy of investigation, to determine how small a diameter of an object might be seen with that instrument. He had, he says, in April 1774, determined a similar question relating to the natural eye; and found that a square area could not be distinguished from an equal circular one till the diameter of the latter came to subtend an angle of 2′ 17″; but, as he did not think it right to apply the same conclusions to a telescopic view of an object, he, in order to

determine the first-mentioned question, made a series of experiments, of which the following is a summary account.

Dr. Herschel's first experiment was made upon the heads of pins, the size of the largest of which was ·1375, and that of the smallest ·0425 of an inch. These pins were placed at the distance of 2407·85 inches from the centre of the object-mirror of the author's ten-feet telescope; the focal length of the mirror of which, on Arcturus, is 119·64 inches, but on the objects above mentioned 125·9.

Dr. Herschel soon found that none but the smallest of those objects could, at the distance at which they were placed, be of any use; and that an object of that size, namely, ·0425 of an inch, may be easily seen, in his telescope, to be a round body when the magnified angle under which it appears is 2′ 18″·9, and that with a high power a part of it, subtending an angle of 0″·364, may be conveniently perceived.

In the second experiment, Dr. Herschel made use of globules of sealing-wax. These globules were of different sizes, from ·0466 to ·00763: and the result of the experiment was, that, with a globule so small as ·00763 of an inch of a substance not reflecting much light, the magnified angle must be between 4 and 5 minutes before we can perceive it to be round. But it also appears that a telescope, with a sufficient power, will show the disc of a faint object when the angle it subtends, at the naked eye, is no more than 0″·653.

The third experiment was made with globules of silver, formed by running the ends of very fine silver wires into the flame of a candle. The size of these globules was from ·03956 to ·00556, and the distance of these objects from the mirror of the telescope was increased to 2370·5 inches. By this experiment it was found that the telescope acted very well with a high power, and would show an object, subtending only 0″·484, so large, that it might be divided into quarters of its diameter.

The fourth experiment was made with globules of pitch, bees' wax, and brimstone. From the two first-mentioned substances, no satisfactory inference could be drawn; but when four globules of brimstone, the sizes of which were ·00962, ·009125, ·00475, ·002375, were viewed with a power of 522·7, the three first appeared round; the fourth was invisible till a dark blue paper was placed a few inches behind it. The angle it subtended was 0″·207.

The fifth experiment consisted in observing the globules of sealing-wax and those of silver at a distance still greater, namely, 9620·4 inches, with a power of 502: the smallest globules of the former substance were invisible at that distance; but all the silver globules except the smallest (which, having met with an accident, could not be examined,) appeared round.

The sixth experiment was made by fixing some of the silver globules on a post, and illuminating them, by holding a lantern against them: with a power of 522·7 they were all seen perfectly well, but the light thrown on them was not sufficient to allow of making angular experiments upon them.

Dr. Herschel then, in order to investigate the causes of the spurious diameters of celestial objects, relates some observations made upon several of the fixed stars, from which he deduces the following inferences:—

1. That the spurious diameters of the stars are larger than the real ones, which are too small to be seen.

2. That the spurious diameters are of different sizes; but that, under the same circumstances, their dimensions are of a permanent nature.

3. That the spurious diameters are differently coloured, and that these colours are permanent when circumstances are the same.

4. That these diameters are proportionally lessened by increasing the magnifying power, and increase when the power is lowered.

5. That the above-mentioned increase and decrease is not inversely as the increase and decrease of the magnifying power, but in a much less ratio.

6. That the magnifying power acts unequally on spurious diameters of different magnitudes; less on the large diameters, and more on the small ones.

7. That when the aperture of the telescope is lessened, it occasions an increase of the spurious diameters; and when increased, reduces them.

8. That this increase and decrease is not proportional to the diameters of the stars, but that an alteration in the aperture of the telescope acts more upon small spurious diameters and less upon large ones.

9. That stars which are extremely small lose their spurious diameters, and become nebulous.

10. That many causes will influence the apparent diameter of the spurious discs of the stars; but that, with a proper regard to those causes, the conclusion already drawn, that under the same circumstances their dimensions are permanent, still remains good.

A number of experiments then succeed on the spurious diameters of terrestrial objects. The first series of these were made by means of the silver globules already mentioned. The inferences drawn from them are similar to those drawn from the observations of celestial objects, except that the spurious discs of terrestrial objects, contrary to what happens with celestial ones, are smaller than the real discs, and that they are apt to be lost for want of proper illumination, but do not on that account change their magnitude.

Similar experiments were made with drops of quicksilver, the results of which differed so little from those of the experiments with globules of silver, as not to require any further description of them.

After two preparatory experiments, one with black and white circles, which showed that no material deception can take place in estimating by such circles, on account of their colour, and another, which showed that no difference in the apparent size of the globules was produced by a different mode of illuminating them in the microscope, Dr. Herschel proceeded to measure the spurious disc of one

of the globules. For this purpose, he viewed it with apertures of different dimensions and of different kinds. Some of them being of the usual annular opening or outside rim, from 6·5 to 8·8 inches, *which reflected less than half the light of the mirror*, produced a spurious disc less than ·18 in diameter; while the whole light of the mirror gave a disc of ·31: he thinks it fair to conclude, that it is not the quantity of light, but the part of the mirror from which it is reflected, that we are to look upon as the cause of the magnitude of the spurious discs of objects; and this, he says, points out an improved method of putting any terrestrial disc, that we suspect to be spurious, to the test: for the inside rays of a mirror will increase the diameter of those discs; but the outside rays alone will have a greater effect in reducing it, than when the inside rays are left to join with them.

Dr. Herschel then placed two other globules at a small distance from each other, without having previously measured either their size or the distance between them. Upon viewing them with a power of 522·7, they appeared in the shape of half-moons; he estimated the vacancy between the cusps to be one fourth the diameter of the largest; and found afterwards, on measuring the diameters and distance, that his estimation did not differ $\frac{1}{1000}$th of an inch from the truth. In a second experiment the difference between the real and the estimated distance was still less.

In order to ascertain whether these half-moons were real or spurious, Dr. Herschel viewed them first with the inside rays of the mirror, then with the outside rays, and lastly, with the whole mirror open, but no alteration in the distance of the lunes could be perceived. He then divided the aperture of the mirror into two parts, one from 0 to 4·4 inches, the other from 4·4 to 8·8; and found, on measuring the spurious diameter of a globule, that with the inside rays it was ·40; with the whole mirror open it was ·31; and with the outside rays it was ·22.

From this, he says, we may conclude, that the diameters given by the inside rays, by all the mirror open, and by the outside rays, are in an arithmetical progression; and that the inside rays will nearly double the diameter given by the outside.

α Lyræ being then examined in the same manner, its spurious disc was found to be small with the outside rays; with the whole mirror open it was larger; and with the inside rays it was largest.

The double star *α* Geminorum was then viewed with a power of 410·5; with the outside rays they appeared unequal, and $1\frac{1}{4}$ diameter of the largest asunder; with the whole mirror open they were more unequal, and $1\frac{1}{2}$ diameter of the largest asunder; with the inside rays they were very unequal, and $1\frac{7}{8}$ of the largest asunder.

The foregoing experiments show, the author says, that if it had not been known that the apparent discs of the stars were spurious, the application of the improved criterion of the aperture would have discovered them to be so; and that, consequently, the same improvement is perfectly applicable to celestial objects.

Dr. Herschel, having made these preliminary experiments, proceeds to apply them to investigate the nature and magnitude of the star lately discovered by Mr. Harding. A regular series of observations on this star are detailed, beginning on the 29th of September, and ending on the 11th of October. Of these we must necessarily confine ourselves to mention merely the general result with the conclusions deduced by the author from the whole of the investigation. These conclusions are as follows:—

1. A ten-feet reflector will show the spurious or real discs of celestial and terrestrial objects, when their diameter is one fourth of a second; and in favourable circumstances that diameter may be divided, by estimation, into two or three parts.

2. A disc of the above diameter, whether spurious or real, to be seen as a round well-defined body, requires a magnifying power of 500 or 600, and must be sufficiently bright to bear that power.

3. A real disc of half a second in diameter will be so magnified by the above-mentioned power, that it may be easily distinguished from a spurious one of equal size, the latter not being affected by magnifying power in the same proportion as the former.

4. The different properties of the inside and outside rays of a mirror, with regard to the appearance of a disc, will show whether it is real or spurious, provided its diameter is more than one-fourth of a second.

5. When discs, either spurious or real, are less than one fourth of a second in diameter, they cannot be distinguished from each other, because the magnifying power is not sufficient to make them appear round and well defined.

6. The same kind of experiments are applicable to telescopes of different sorts and sizes, but will give a different result for the quantity here stated at one fourth of a second, being more when the instrument is less perfect, and less when it is more so.

The general results of Dr. Herschel's observations on Mr. Harding's newly discovered celestial body, to which the name of Juno has been given, are,—

1. That it is in every respect similar to those discovered by Mr. Piazzi and Dr. Olbers, so that Ceres, Pallas, and Juno, are certainly three individuals of the same species.

2. That these bodies (the last of which appears to be the smallest,) are incomparably smaller than any of the planets; for a telescope that will show a diameter of one fourth of a second, will not determine whether their discs are real or spurious, although a power of more than 600 has been applied to each of them.

3. That the criterion of the apertures of the mirror has, on account of the smallness of the object, been equally unsuccessful; every method that has been tried only proving their resemblance to small stars.

4. That the definition of the term asteroid, formerly given by Dr. Herschel, will equally express the nature of Juno, which, on account of its similar situation between Mars and Jupiter, and its departure from the general condition of planets, by the smallness of its

disc, and the great inclination and eccentricity of its orbit, may also be considered as a true asteroid.

Dr. Herschel concludes by observing, that the specific difference between planets and asteroids appears now, by the addition of a third individual of the latter species, to be more fully established; and that circumstance, he thinks, has added more to the ornament of our system, than the discovery of another planet could have done.

An Essay on the Cohesion of Fluids. By Thomas Young, *M.D. For. Sec. R.S.* Read December 20, 1804. [*Phil. Trans.* 1805, *p.* 65.]

Dr. Young's principal objects in this paper are to reduce the phenomena of the capillary action of fluids to the general law of an equable tension of their surfaces; to investigate the properties of the curves resulting from this law; to determine the magnitude of the apparent adhesion of solids to fluids, and the cohesion of moistened solids; and to show in what manner the law itself is probably derived from the fundamental properties of matter.

Dr. Young observes, that a fluid which is not capable of wetting a given solid, forms with it an angle of contact which is constant in all circumstances; that the curvature of the surface of a fluid, or the sum of the curvatures, where the curvature is double, is always proportional to the elevation or depression with respect to the general surface, and that the curve itself admits, in all cases, an approximate delineation by mechanical means, but is not always capable of being easily subjected to calculation. When, however, the curvature is simple, the direction of the surface, at any given height, admits a correct determination. Hence the elevation of a fluid in contact with a given surface, whether vertical, horizontal, or inclined, is deduced from its ascent between plates, or in a tube, of the same substance; and the result is shown to agree with experiments. Thus, taking $\frac{1}{19}$th of an inch for the diameter of a tube, in which water rises to the height of an inch, it is inferred that the apparent adhesion of water, to a square inch of any horizontal surface capable of being wetted by it, must be 50½ grains, which is only half a grain more than the result of Taylor's experiments. The adhesion of alcohol, and of sulphuric acid, as measured by Achard, are also found to agree with the ascent of those fluids in capillary tubes. Lord Charles Cavendish's table of the depression of mercury in barometer tubes, is compared with the same principles by means of diagrams constructed for each particular case; and the apparent adhesion of the surface of mercury to glass, as well as the depth of a portion of mercury spread on a plate of glass, is deduced from these measures, and is shown to agree with experiments. The observations of Morveau, on the attraction of the different metals to mercury, are also discussed; and some remarks are made on the magnitude of drops of various substances.

The hydrostatic actions of these elevations and depressions of fluids are such as to afford a ready explanation of the attractions

and repulsions of floating bodies: these attractions are found to vary ultimately in the inverse ratio of the squares of the distances; and they appear to be the same as are found to cause an apparent cohesion between any moistened surfaces nearly in contact: the magnitude of this cohesion, as measured, in a particular case by Morveau, being found to agree with the calculation of the effect of capillary action.

The attraction of a drop of a fluid towards the line of contact of two plates of glass, which was found by Hawkesbee to vary nearly in the inverse ratio of the square of the distance of the plates, was supposed by Newton to indicate an immediate cohesive force, varying in the simple inverse ratio. But Dr. Young has shown that the fundamental law of the equable tension of the surface is sufficient to explain this phenomenon, and to remove the apparent irregularity in the laws of cohesive forces.

The equable tension of the surface is shown to be a consequence which may be mathematically deduced from the existence of a stable equilibrium between the forces of repulsion and of cohesion, which is a necessary condition of liquidity, as the repulsive force always varies more rapidly than the cohesive force. The mutual attractions of solids and fluids are then considered; and Dr. Young agrees with Clairaut, although upon different grounds, in affirming that a fluid will be elevated when in contact with any solid of more than half its attractive density. The tension of the common surfaces of a solid and a fluid, or of two continuous fluids, is supposed to be proportional to the difference of the attractive densities; and this supposition is confirmed by some observations, with which the paper is concluded, on the phenomena of oily substances floating on water.

Concerning the State in which the true Sap of Trees is deposited during Winter. In a Letter from Thomas Andrew Knight, *Esq. to the Right Hon. Sir* Joseph Banks, *Bart. K.B. P.R.S.* Read January 24, 1805. [*Phil. Trans.* 1805, *p.* 88.]

This paper may be considered as a continuation of Mr. Knight's former communications respecting the motion of the sap in trees. Du Hamel, and other subsequent naturalists, have shown that trees contain two kinds of sap; and the chief purpose of Mr. Knight's paper is to prove that one of them (called by Du Hamel *suc propre*, and by Mr. Knight the true sap,) is generated in the leaf; and that this fluid, in an inspissated state, or some concrete substance deposited by it, exists during the winter in the alburnum, from which substance, dissolved in the ascending aqueous sap, is derived the matter which enters into the composition of the new leaves in the spring. To the above-mentioned deposition, Mr. Knight attributes the well-known superiority of winter-felled wood, which superiority has generally been supposed owing merely to the absence of the sap at that season.

Du Hamel has remarked, that trees perspire more when the leaves

are full grown, and when the annual shoots have ceased to elongate, than at any earlier period. This energy in the powers of vegetation must certainly, Mr. Knight thinks, be employed in some very important operation. He has observed that the produce of his meadows has been greatly increased when the herbage of the preceding year had been left till the end of autumn, on ground that had been mowed early in the summer; from which he has been led to imagine, that leaves are employed during the latter part of the summer in the preparation of matter calculated to afford food to the buds and blossoms of the succeeding spring.

In order to determine whether the foregoing opinions were well founded or not, Mr. Knight made the following experiments.

Having made incisions in the trunks of sycamore and birch-trees (some of these incisions being close to the ground, others at the elevation of seven feet), he found that the sap obtained from the sycamore close to the ground, was of the specific gravity of 1·004, while that obtained at the height of seven feet had a specific gravity of 1·008. The sap of the birch was somewhat lighter; but the increase of specific gravity, at different elevations, was comparatively the same. The sap of both these trees, when extracted near the ground, was almost void of taste; but when obtained at a greater height, it was sensibly sweet. In one instance it was extracted from the sycamore-tree at the height of twelve feet; it was then very sweet, and its specific gravity was 1·012.

Mr. Knight then made an experiment to compare the sap obtained from a recent incision with that obtained from an old one. He found that the sap from an old incision was reduced in specific gravity to 1·002, while that from the recent incision continued at 1·004, as before. These incisions were made in a sycamore-tree, and were close to the ground.

Some observations then follow on the variation in the specific gravity of the alburnum at different seasons. After taking every precaution to avoid error, the author found the specific gravity of winter-felled oak to be 0·679, and that of summer-felled oak to be 0·609, after they had both been immersed five minutes in water. This difference appearing to Mr. Knight very considerable, he repeated the experiment several times, but found no reason to suspect any error in it; and upon measuring pieces of both kinds of wood, which were equal in weight, it appeared that the winter-felled pieces were much less than the others. The more recently formed layers of winter-felled wood had a specific gravity of 0·583; that of the summer-felled wood was only 0·533. In another experiment the former was 0·588, the latter 0·534.

On pouring boiling water on equal quantities of summer- and of winter-felled wood, it appeared that the latter communicated a much deeper colour to the water than the former; it also raised the specific gravity of the water to 1·002; the specific gravity of the other infusion was 1·001.

Mr. Knight thought he had reason to believe that the matter de-

posited in the alburnum sometimes remains unemployed during several successive years; he therefore cut off, in the winter, all the branches of a large and very old pear-tree, at a small distance from the trunk, and pared off, at the same time, all the lifeless external bark. No marks of vegetation appeared till the beginning of July following, when numerous buds and leaves, of large size, appeared; and in autumn every part was covered with very vigorous shoots. The number of leaves appeared to Mr. Knight to exceed very much the whole of those the tree had borne in the three preceding years.

Mr. Knight says that he has repeated, with success, the experiments of Bonnet and Du Hamel, and that he is in possession of many other facts which, like those experiments, tend to prove that seedling trees depend, at first, entirely on the nutriment afforded by the cotyledons; and that they are greatly injured, and often killed, by being put to vegetate in rich mould. He thinks there is very decisive evidence that bulbous and tuberous-rooted plants contain within themselves the matter which subsequently composes their leaves; also that it appears extremely probable, that the blossoms of trees receive their nutriment from the alburnum, particularly as the blossoms of many plants precede their leaves.

Mr. Knight also thinks the existence of a vegetable circulation, though denied by many eminent naturalists, must be admitted. He supposes that when a seed is placed in a proper situation for vegetation, water is absorbed by the cotyledons, and a young radicle is emitted. This increases in length, by the addition of new matter to its apex, not by any general distension of its vessels or fibres; which new matter appears, from the experiments of Bonnet and Du Hamel, to descend from the cotyledons. The first motion, therefore, of the fluids is downwards, towards the point of the root; and the vessels which carry those fluids are similar to those which are subsequently found in the bark. In support of this opinion, he mentions some observations he has made on the progressive changes which take place in the radicle of the horse-chestnut. From these it appears, that when the roots were considerably elongated, and not till then, alburnous tubes were formed, and that as soon as these tubes had acquired a sufficient degree of firmness, they appeared to begin their office of carrying up the aqueous sap; at which time, and not sooner, the leaves of the plumula expanded. When the leaf has attained its proper growth, it seems to perform precisely the office of the cotyledon, being fed by the alburnous tubes and central vessels; and the true sap is discharged from the leaf, as it was previously from the cotyledon, into the vessels of the bark. Here one part of it produces the new layer of wood (or new epidermis when that is to be formed), and the remaining part enters the pores of the wood already formed, and mixes with the ascending aqueous sap.

The author thinks it probable that the true sap undergoes a considerable change on its mixture with the ascending aqueous sap, as in the sycamore; it was found to become more sensibly sweet in its progress in the root, in the spring, although he could never detect

the slightest degree of sweetness in decoctions of the wood in winter. He therefore is inclined to believe, that the saccharine matter is generated by a process similar to that of the germination of seeds; and that the said process is always going on during the spring and summer; but that towards the conclusion of the summer, the true sap simply accumulates in the alburnum, and thus adds to the specific gravity of winter-felled wood, and increases the quantity of its extractive matter. He says also, that he has some reasons for thinking that the true sap descends through the alburnum, as well as through the bark; and that he has been informed, that if the bark be taken from the trunks of trees in the spring, and such trees be suffered to grow till the following winter, the alburnum acquires a great degree of hardness and durability.

Mr. Knight concludes by observing, that he conceives himself to be in possession of facts, which prove that both buds and roots originate from the alburnous substance of plants, and not, as he believes is generally supposed, from the bark.

On the Action of Platina and Mercury upon each other. By Richard Chenevix, *Esq. F.R.S. M.R.I.A.* &c. Read January 10, 1805. [*Phil. Trans.* 1805, *p.* 104.]

Mr. Chenevix, in the month of May 1803, presented to the Royal Society a paper, which was printed in the Philosophical Transactions for that year, respecting the nature of a metallic substance which had been offered to the public as a new simple metal, under the name of Palladium. In that paper he not only attempted to prove that the said substance, instead of being a simple metal, was merely a compound of platina and mercury, but he also described certain processes by which he had been enabled to produce it. He now expresses his mortification to learn that the processes he there recommended, as the least likely to fail, have been generally unsuccessful; and confesses he has reason to believe "that the nature of palladium is considered by most chemists as unascertained, and that the fixation of mercury by platina is by many regarded as visionary."

In France, he says, the compound nature of palladium has been more generally credited; M. Guyton, who was appointed by the National Institute to make a report upon Mr. Chenevix's experiments, having repeated some of them, and having been led by the results to the same general conclusions as Mr. Chenevix.

Messrs. Fourcroy and Vauquelin also made some experiments upon the subject; but as about this time a new metal had been discovered in crude platina by Mons. Descotils, the above-mentioned chemists were led to suppose it probable that the new metal was concerned in the production of palladium; and finally declared, as their opinion, that the substance called palladium does not contain mercury, but is formed of platina and the new metal of M. Descotils. Mr. Chenevix adduces several arguments to show that this opinion is not well founded; and in the latter part of his paper, he says, that

in order to ascertain the matter, he observed the methods recommended by those chemists for obtaining pure platina, but did not perceive any difference in the facility with which either kind of platina combined with mercury.

Mr. Chenevix then adverts to the experiments of Messrs. Rose and Gehlen, who attempted to repeat some of the processes described by him for the formation of palladium. The experiments of those gentlemen were unsuccessful; but this, Mr. Chenevix considers as not militating against his experiments, as he shows that the processes made use of by them, though meant to be an exact imitation of his, were, in fact, materially different. The same gentlemen, Mr. Chenevix says, seem to question his having fused platina, as they could not succeed, although they exposed it to a heat, the degree of which Wedgwood's pyrometer ceased to mark. Upon this Mr. Chenevix remarks, that they do not mention their having made use of any flux; whereas he employed borax for that purpose. He then describes, at full length, the method used by him, which consists in filling a Hessian crucible with lamp-black pressed hard together, and placing the platina, surrounded by borax, in the centre of the lamp-black, at the bottom of which there is previously formed a cavity to receive the platina when fused.

Mr. Richter also attempted to make palladium by the process which Mr. Chenevix describes as the best for that purpose, but failed. He was, however, convinced by his trials, that "mercury is capable of entering into combination with platina, so that it cannot afterwards be separated by fire."

It appears also that Mr. Tromsdorff, and likewise Mr. Klaproth, have made some fruitless attempts to obtain palladium. As these gentlemen, as well as Messrs. Rose and Gehlen, and Mr. Richter, seem disappointed at their want of success, Mr. Chenevix takes occasion to observe, that they appear to have placed a reliance upon his processes, which his words did not authorize; and says that his paper, "as far as regards palladium, is rather a narrative of fruitless attempts, than a description of an infallible process, and more likely to create aversion to the pursuit, than to inspire a confidence of success."

The compound nature of palladium, Mr. Chenevix thinks, has received some support from the galvanic experiments of Mr. Ritter, who found its galvanic properties different from what they ought to be, upon the supposition of its being a simple metal.

As a further excuse for the failure that has attended the repetition of his processes, Mr. Chenevix mentions that Prof. Lampadius, a few years ago, "in distilling some substances that contained sulphur and charcoal, obtained a peculiar liquid, which he called sulphur-alcohol; and that, after many fruitless attempts to procure the liquid a second time, he abandoned his researches. Messrs. Clement and Desormes, however, some time after obtained this liquid, but were equally unsuccessful in their numerous attempts to produce it again. In February last, Professor Lampadius accidentally discovered, and

has published, a method of obtaining the above-mentioned liquid, which never fails.

"By taking the reasoning on this subject in its widest extent, Mr. Chenevix thinks we shall be led to conclude, that metals may exercise an action upon each other, even in their metallic state, capable of so altering some of their principal properties, as to render the presence of one or more of them not to be detected by the usual methods. In this is contained the possibility of a compound metal appearing to be simple. But to prove this proposition must be a work of great time and perseverance, and can only be done by considering, singly and successively, the different cases which it contains, and by instituting experiments upon each."

As an example of the foregoing position, and to prove that mercury and platina act upon each other so as to disguise the properties of both, Mr. Chenevix states, that when a solution of green sulphate of iron is poured into a solution of platina, no precipitate, nor any other sensible change, ensues; but if a solution of silver, or of mercury, be added, a copious precipitate, in part metallic, takes place. He has tried to produce the same effect with other metals and platina, but has not observed anything similar. From this he thinks it fair to conclude, that "whenever a solution of platina is precipitated, in a metallic state, by a solution of green sulphate of iron, either silver or mercury is present. He adds, that the precipitation of a mixed solution of platina and silver, requires no further caution than to free the salt of platina, as much as possible, from muriatic acid."

Mr. Chenevix then makes some observations, which cannot well be abridged, on the precipitation of platina by mercury; but which show that "the state of oxidizement of the latter metal, as well as the acid in which it is dissolved, produce a considerable modification in the result." It then occurred to him, that "a method of uniting platina and mercury, without the intervention of any other metal, or of any substance except the solvents of these metals, might be accomplished, as in the case of silver and platina:" and he describes an experiment, which shows that when the mercury is at its minimum of oxidizement in nitric acid, the addition of green sulphate of iron is superfluous. But, on the contrary, if "mercury be raised to its *maximum* of oxidizement in nitric acid, no precipitation occurs till the green sulphate of iron is added."

Mr. Chenevix also makes a variety of observations respecting the complicated affinities which take place when the muriates of the above metals are employed. These we shall pass over, and proceed to notice those experiments which form the principal object of this paper. From the first experiment it appears, that if a solution of highly oxidized nitrate of mercury is poured into a mixed solution of platina and green sulphate of iron, a muriate of mercury is formed, and also a metallic precipitate; the former is retained in solution; the latter weighs more than the original quantity of platina, even after nitric acid has been boiled repeatedly, and in large quantities, upon it. By exposure to heat, he acknowledges that little more,

in general, is left, than the original weight of the platina; and admits that even a diminution of weight may be sometimes observed. Before the precipitate has been exposed to heat, it may be dissolved in nitro-muriatic acid more easily than platina itself.

The second experiment is to show that when a mixed solution of platina and mercury is precipitated by metallic iron, a precipitate nearly equal to the sum of the two former metals is generally obtained, the properties of which appear to be similar to those of the precipitate obtained in the first experiment.

We learn, from the third experiment, that when an amalgam of platina, formed by means of the ammoniacal muriate of that metal, according to the method of Count Mussin Pushkin, is exposed to heat, a metallic powder remains, which is soluble in nitro-muriatic acid, and affords a copious precipitate by means of green sulphate of iron.

The fourth experiment states, that if sulphur is added to the ingredients used in the formation of the above-mentioned amalgam, and the whole treated as before, the precipitate caused by green sulphate of iron is more considerable.

The fifth experiment informs us, that if sulphur is rubbed with ammoniacal muriate of platina, the mixture may be melted on a sand bath. If mercury is then added to the melted mass, and the whole exposed to a strong fire, a button remains, which, being dissolved in nitro-muriatic acid, gives a precipitate, as before, with the green sulphate of iron.

In the sixth experiment we are told, that if sulphuretted hydrogen is passed through a mixed solution of platina and mercury, and the precipitate afterwards melted with borax, the button will not contain any sulphur. Green sulphate of iron causes a precipitate in the solution of this button.

The seventh experiment serves to show that phosphate of ammonia, when added to a solution of platina and mercury, causes a precipitate, the solution of which is acted upon by green sulphate of iron.

By the eighth experiment it appears, that if the precipitate formed by adding nitrate of mercury, at the minimum of oxidizement, to muriate of platina be washed, reduced, and afterwards dissolved in nitro-muriatic acid, another precipitate may be produced by means of green sulphate of iron.

The ninth experiment relates to the action of recent muriate of tin, which Mr. Chenevix says is one of the most delicate tests in chemistry, detecting the presence of mercury. If a single drop of neutralized nitrate or muriate of mercury is put into 500 grains of water, the addition of muriate of tin causes the liquor to become turbid, and to assume a smoke-gray colour. And even if the above liquor is diluted with ten times its weight of water, the effect is still sensible. But if recent muriate of tin is poured into a solution, not too much concentrated, of platina and mercury, it can hardly be distinguished from a simple solution of platina. If, however, too much

mercury be present, the excess is acted upon as in the case of mercury, the liquor assuming a darker colour than with platina alone.

From the above experiments, Mr. Chenevix infers that mercury can act upon platina, and confer upon it the property of being precipitated, in a metallic state, by green sulphate of iron. The first and second experiments prove, he says, that platina can protect mercury from the action of nitric acid; and also that mercury increases the action of nitro-muriatic acid upon platina. The third, fourth, fifth, sixth, seventh, and eighth experiments show that mercury can combine with platina, in such a manner as not to be separated from it by the degree of heat necessary to fuse the compound. The eighth experiment proves that the action of mercury upon platina is not confined to the metallic state, but that these metals can combine and form an insoluble triple salt, with an acid that produces a very soluble compound with platina alone. The ninth experiment shows that platina can retain in solution a certain quantity of mercury, and prevent its detection by a substance which acts powerfully when platina is not present.

Mr. Chenevix admits that one or two of the above-mentioned experiments appear to contradict some of those he described in his former paper on Palladium; for in the present experiments, "platina protects mercury against the action of nitric acid, whereas in palladium the mercury is not only acted upon itself, but contributes to the solution of platina in the same acid." The discussion of these objections, Mr. Chenevix says, he shall defer to another opportunity. He also acknowledges that there is some incorrectness in his former paper, with respect to the proportional quantities of the ingredients that enter into the composition of the metallic precipitate formed by means of green sulphate of iron; and after making various remarks on that head, states the mean result to be about 17 parts of mercury, and 83 of platina, when the specific gravity of the compound was about 16.

Mr. Chenevix concludes by observing, "that he has as yet seen no arguments of sufficient weight to convince him, in opposition to experiment, that palladium is a simple substance. Nothing is more probable, he says, than that nature may form the alloy called palladium, and even form it better than we can do; and he thinks that the amalgamation to which platina is submitted before it reaches Europe, is sufficient to account for its containing a small portion of palladium." With respect to the failure that has happened in the attempts of others to make palladium, he says he is himself too well "accustomed to such failure not to believe that it will happen, even in well conducted trials;" but he considers four successful experiments, which were not performed in secret, as a sufficient answer to that objection.

The experiments above related tend, in his opinion, to confirm his former results; but he allows that he can prescribe no other means for success than perseverance; and as Messrs. Fourcroy, Vauquelin,

and Richter, have promised to continue their researches on the subject, some great and important fact must, he thinks, issue from their labours.

An Investigation of all the Changes of the variable Star in Sobieski's *Shield, from five Years' Observations, exhibiting its proportional illuminated Parts, and its Irregularities of Rotation; with Conjectures respecting unenlightened heavenly Bodies. By* Edward Pigott, *Esq. In a Letter to the Right Hon. Sir* Joseph Banks, *K.B. P.R.S.* Read February 7, 1805. [*Phil. Trans.* 1805, *p.* 131.]

Mr. Pigott, some years ago, presented to the Royal Society a paper, which is printed in the Philosophical Transactions for the year 1797, on the periodical changes of brightness of two fixed stars. The first part of the present paper consists of a series of observations made since those of the former paper, during the space of nearly five years, on one of the said stars, namely, that in Sobieski's Shield. These observations are fully detailed in various tables; and mean results are deduced from the observations given in the former paper, and from those described in the present one. The results are as follows:—Rotation of the star on its axis, 62 days.—Duration of brightness at its maximum, without any perceptible change, 9½ days.—Duration of brightness when it does not attain its usual brightness, 20 days.—Duration of brightness at its minimum, without any perceptible change, 9 days.—Ditto when it does not decrease so much as usual, 20 days.—Decrease in time, from the middle of its full brightness to the middle of its least, 33 days.—Increase of time, from the middle of its least brightness to the middle of its full, 29 days.—Extremes of its different degrees of brightness, 5th to 9th magnitude.—Mean of its usual variation, 5th to 6th magnitude.

In the second part of this paper Mr. Pigott proceeds to examine some of the other phenomena belonging to this star, particularly one which, he says, is common to most of the variables, and likewise in some degree to our sun, namely, that the times of their periodical returns of brightness are, in general, irregular. In hopes of making some discovery respecting the cause of these irregularities, or at least of assisting future astronomers to form some opinion respecting them, Mr. Pigott made a series of observations on the star here treated of, beginning in October 1795, and ending in October 1801. These observations are detailed at full length in two tables; and it appears from them, that the periodical returns of brightness are uncommonly fluctuating, and that the differences between the extremes are very considerable. Mr. Pigott then, by way of explanation, offers the following opinions and inferences.

1st. That the bodies of the stars are dark and solid.

2ndly. That their real rotation on their axes is regular, following uniform impulses.

3rdly. That the surrounding medium does, at times, generate and

absorb its luminous particles, in a manner nearly similar to that which Dr. Herschel has supposed to take place with regard to the sun's atmosphere.

4thly. That as the variable star in Sobieski's Shield is occasionally diminished in appearance to the 6th or 7th magnitude, or even to a smaller magnitude, it appears that these luminous particles are but sparingly dispersed in its atmosphere.

5thly. He asks, may we not with much plausibility represent such luminous particles as spots, somewhat circular, and of no great extent.

6thly. That the principal bright parts are but slight patches, may, he says, be presumed, from the perpetual changes they undergo, and also from such changes being very visible to us.

7thly. He thinks we may obtain some idea of the relative situation or intervals between these bright parts, by the observations of the increase and decrease of brightness, as thereby the changes and times elapsed are pointed out.

Mr. Pigott says he has tried, practically, the effect of the above suppositions, by placing small white spots on a dark sphere, which sphere being turned round, represented the various changes as nearly as could be expected. Of these changes several views are given, accompanied with some observations on variable stars in general; in the course of which the author supposes it probable that many stars have lost their light, and that there are many others which have never shown a glimpse of brightness. He even asks, whether we may not suppose the number of these unenlightened stars equal to that of those endowed with light? If so, he thinks that by being collected together in clusters, as in the Milky Way, they must intercept all more distant rays; and if free from any intervening lights, must appear as dark spaces in the heavens, similar to what has been observed in the southern hemisphere.

Mr. Pigott, at the conclusion of his paper, says he thinks there are strong reasons to believe that the sun's luminous appearance has been at times considerably diminished; also, that he has little hesitation in conceiving it may, at some future period, be reduced to small patches.

An Account of some analytical Experiments on a mineral Production from Devonshire, consisting principally of Alumine and Water. By Humphry Davy, *Esq. F.R.S. Professor of Chemistry in the Royal Institution.* Read February 28, 1805. [*Phil. Trans.* 1805, *p.* 155.]

The mineral, of which an account is here given by Mr. Davy, was found many years ago by Dr. Wavel, in a quarry near Barnstaple. It was then considered as a kind of zeolite; but Mr. Hatchett, who visited the place in the year 1796, describes it as filling some cavities and veins in a rock of soft argillaceous schistus; and from that circumstance concluded, that it most probably did not belong to the

above-mentioned genus. Dr. Babington, from its physical characters, and from some experiments made on its solution in acids, ascertained that it was a mineral substance not yet described, and that it contained a considerable portion of aluminous earth.

This mineral is generally found in small hemispherical groups of crystals, composed of filaments radiating from a common centre, and inserted on the surface of the schistus: sometimes, however, it forms small veins of irregularly disposed prisms. It is of a white colour, having sometimes a tinge of gray, or of green; and, when beginning to be decomposed, of yellow. Its lustre is silky; it is generally almost opake, but sometimes semi-transparent. It is fragile; but its small fragments are so hard, as to be capable of scratching agate. It has no smell when breathed upon; it has not any taste, nor does it adhere to the tongue till it has been strongly ignited. It does not become electrical, or phosphorescent, by heat or friction; nor does it decrepitate before the blowpipe, but loses its hardness, and becomes quite opake. Its specific gravity, Mr. Davy thinks, does not exceed 270, water being considered as 100.

The white and semi-transparent specimens of this substance are soluble in the mineral acids, and also in fixed alkaline lixivia, without effervescence; but when coloured or opake specimens are exposed to alkaline lixivia, a small part remains undissolved.

A small transparent piece, by being exposed to the greatest heat of a forge, had its crystalline texture destroyed, and was rendered opake, but was not fused. It now had lost more than one-fourth of its weight, and adhered strongly to the tongue; neither water nor alcohol had any effect on this mineral. When exposed, in a glass tube, to a heat of from 212° to 600°, it gave out an elastic vapour, which, when condensed, was a clear fluid, having a slightly empyreumatic smell, but not differing in taste from pure water.

The solution of this substance in sulphuric acid produced crystals in thin plates, which had the properties of sulphate of alumine, and from which, when re-dissolved and mixed with potash, octahedral crystals of alum were obtained.

The solid matter precipitated from the solution of this substance in muriatic acid, was not acted upon by carbonate of ammonia, consequently it did not contain glucine or yttria.

Several experiments were made on the opake and coloured varieties of this mineral, from which it appears that the substances which cause these varieties, are calcareous earth, manganese, and oxide of iron.

Mr. Davy then proceeded to the analysis of the mineral. For this purpose he made use of the whitest and most transparent pieces he could obtain. The particulars of this analysis we shall pass over; and shall merely state that, according to its general results, 100 parts of the mineral contain, of alumine 70, of lime 1·4, of fluid 26·2, the loss amounting to 2·4; which loss Mr. Davy is inclined to attribute to some fluid remaining in the stone after the process of distillation,

having found, from several experiments, that a red heat is not sufficient to expell all the matter capable of being volatilized.

Mr. Davy then made some experiments to determine whether any portion of fixed alkali existed in this mineral, but no indications of such alkali could be observed.

The fluid obtained by distilling several different specimens of this mineral was similar in its properties; the only test of the presence of acid matter in it was litmus paper; and in some instances the effect upon this paper was scarcely perceptible. Mr. Davy made several experiments to determine the nature of the above acid matter, but without success.

It is, however, he says, evident that it is not any one of the known mineral acids: he is also disposed to believe, that it is not an essential component part of the mineral, but that, as well as the oxide of manganese, the oxide of iron, and the lime, it is only an accidental ingredient. Hence the mineral, when in a state of purity, must, he thinks, be considered as a chemical combination of about 30 parts of water, and 70 of alumine.

The diaspore, which has been examined by M. Vauquelin, loses 16 or 17 parts in the 100 by ignition, and contains nearly 80 parts of alumine, and 3 of oxide of iron. It is supposed by M. Vauquelin to be a compound of alumine and water. But its characters are very different from those of the mineral here described; and the nature of the part volatilized by heat has not yet been ascertained.

A mineral similar to that here treated of has been found near St. Austle in Cornwall; and Mr. Davy has been informed that, according to an analysis of it made by the Rev. William Gregor, it appears to consist of similar ingredients.

Dr. Babington has proposed to call this mineral by the name of Wavellite, from the gentleman who discovered it in Devonshire; but if a name founded upon its chemical composition should be preferred, Mr. Davy thinks it may be denominated Hydrargillite.

Experiments on Wootz. By Mr. David Mushet. *Communicated by the Right Hon. Sir* Joseph Banks, *K.B. P.R.S.* Read February 14, 1805. [*Phil. Trans.* 1805, *p.* 163.]

The fine cakes of the kind of steel called Wootz, which form the subject of the present paper, were delivered to Mr. Mushet, for the purpose of examination, by Sir Joseph Banks. Mr. Mushet begins his account of them by giving a very minute description of the form, the grain, and every other external character of these cakes. This description cannot well be abridged, and is too long to be repeated. We shall therefore only say that Mr. Mushet states, as a general remark, that the grain and density of these cakes of wootz were uniformly homogeneous, and free from metallic iron towards the under or round surface, but that they were always the reverse towards the upper side, called by Mr. Mushet the feeder.

The appearances observed upon forging these cakes are then par-

ticularly described, from which Mr. Mushet deduces the following general remarks.

The formation of wootz, he says, appears to him to be in consequence of the fusion of a particular ore, which he supposes to be calcareous, or to be rendered so by a mixture of calcareous earth, along with a portion of carbonaceous matter. The fusion, he thinks, is performed in a clay vessel or crucible, in which vessel the separated metal is allowed to cool. Hence, in his opinion, arises the crystallization that occupies the pits and cells observed in and upon the under or round surface of the cakes.

The want of homogeneity and solidity in these cakes, appears to Mr. Mushet to be owing to the want of a sufficient degree of heat to effect a perfect reduction; and this opinion, he thinks, is strengthened by observing, that those cakes which are the hardest, or which contain the largest portion of carbonaceous matter, and, of course, form the most fusible steel, are always the most solid and homogeneous; while, on the contrary, those cakes which are the most easily cut by the chisel, are in general cellular, and abound with veins of malleable iron. If the natives of the country which produces the wootz were capable of rendering it perfectly fluid, Mr. Mushet thinks they would certainly have run it into moulds, by which, he says, they would have acquired a kind of steel more uniform in its quality, and more fit for the purpose of being worked and applied to the arts.

Some of the cakes here described had, around the feeder, and upon the upper surface in general, evident marks of the hammer. This appearance Mr. Mushet accounts for by supposing, that when the cake was taken from the pot or crucible, the feeder was most probably slightly elevated, and the top of the cake covered in part with small masses of ore, which, from want of a sufficient degree of heat, had not been perfectly fused. These, he thinks, are cut off at a second heating, and the surface then hammered smooth, to make the cakes more fit for sale. Mr. Mushet says he has observed similar appearances in operations of a like nature, where the heat has been insufficient; and that such phenomena sometimes take place in separating crude iron from its ores, when, from its containing an excess of carbon, it is difficult to be fused.

The division of the cakes, by the native manufacturer, he thinks, is done merely to facilitate its subsequent application to the purposes of the artist, and to serve as a test of the quality of the steel.

In order to determine by direct experiment whether wootz owes its hardness to an excess of carbon, Mr. Mushet made some comparative experiments upon the cakes, and upon common cast steel and white cast iron. In operations of this kind, he says, he has always found the proportion of carbon best ascertained by the quantity of lead reduced from flint glass. He therefore mixed a certain quantity of wootz, or of steel, or iron, with three times the weight of pounded flint glass, and exposed the mixture to a heat of 160° of Wedgwood's pyrometer.

The result of these experiments was as follows:—

The wootz of the 1st cake	reduced	0·139 its own weight of lead.
That of the 2nd	——	0·125 —— ——
—— 3rd	——	0·120 —— ——
—— 4th	——	0·156 —— ——
—— 5th	——	0·102 —— ——
Steel containing $\frac{1}{60}$ its weight of carbon		0·094 its own weight of lead.
White cast iron ——	—— ——	0·228 —— ——

From these experiments, the author says, it appears, that wootz contains a greater proportion of carbonaceous matter than the common sorts of cast steel, and that some particular cakes approach very near to the nature of cast iron. This, added to the imperfect reduction, seems to him quite sufficient to account for its refractory nature, and for the want of homogeneity in its texture.

Notwithstanding the above imperfections, Mr. Mushet thinks wootz possesses the radical principles of good steel, and that it is impossible not to have a very high opinion of the excellence of the ore from which it is produced; the possession of which, for the fabrication of steel and bar iron, would be an object of the highest importance. It is, he says, a subject of regret that such a source of wealth cannot be annexed to the dominions of this country; as in that case the East India Company might supply their settlements with an article superior in quality, and inferior in price, to any they send from this country.

Abstract of Observations on a diurnal Variation of the Barometer between the Tropics. By J. Horsburgh, *Esq. In a Letter to* Henry Cavendish, *Esq. F.R.S.* Read March 14, 1805. [*Phil. Trans.* 1805, *p.* 177.]

It appears from Mr. Horsburgh's journal, that in steady weather, within the tropics, a regular elevation and depression of the barometer takes place twice in every twenty-four hours, the greatest depression being about four o'clock morning and evening, and the greatest elevation being from eight in the morning till noon, and from nine or ten in the evening till midnight.

In a letter which accompanies the journal, dated Bombay, April 20th, 1804, Mr. Horsburgh says he has observed, since his arrival in India, that the atmosphere appears to affect the barometer differently at sea from what it does on shore. As a proof of this, Mr. Horsburgh gives a series of observations, made on two barometers, one by Troughton, the other by Ramsden, of which the following is an abstract.

From the time of leaving the Land's End, on April 19th, 1802, the mercury was fluctuating and irregular, till April 29th, lat. 26° N., long. 20° W., when it constantly performed two elevations and two depressions every twenty-four hours. These Mr. Horsburgh calls equatropical motions. From lat. 26° to 10° the difference in the high and low stations of the mercury was not so great as it was from

the latter latitude across the equator, and from thence to lat. 25° S. Within the last-mentioned limits, the difference was very considerable, being generally from five to nine hundredths of an inch, both in the day and the night.

When the ship arrived in lat. 28° S., long. 27° W. (June 7th), the mercury no longer performed the equatropical motions, but was irregular and fluctuating until July 11th, when the ship was in latitude 27° S., long. 51° E. The equatropical motions then took place again, and continued with great regularity while the ship sailed up the Madagascar Archipelago, and across the equator, until the arrival of the ship at Bombay, on the 31st of July.

On the 6th of August the barometers were placed on shore, and, from that day to the 12th, the mercury appeared to have a small tendency towards the equatropical motions; from the latter day to the 22nd of the same month, that tendency was so much diminished as to be generally imperceptible.

On the 23rd of August the barometers were again put on board the ship, which left the harbour of Bombay on the 26th. The mercury then immediately began again to perform the equatropical motions, and continued them, with great uniformity, down the Malabar coast, across the bay of Bengal, in the strait of Malacca, and through the China Sea, until the ship arrived in Canton river, on the 4th of October. The mercury then became nearly stationary, or, if it showed at times a small inclination towards the equatropical motions, such inclination was not by any means so perceptible as at sea.

The ship remained at Canton till the 2nd of December, and, although there appeared at times a slight tendency in the mercury to perform the equatropical motions, the rise and fall was in general so small as to be frequently imperceptible; but, on the departure of the ship from Canton river, the motions instantly took place, and continued until the ship arrived in Bombay harbour, on the 11th of January 1803.

From that day to the 23rd of May, the ship remained at Bombay; and during the whole of that time, no tendency towards the equatropical motions worth noticing could be perceived. On the above-mentioned day, the instant the ship left the harbour, the motions of the mercury again took place, and continued, but not always with equal regularity, until the 2nd of July, when the ship again entered Canton river.

During the time the ship continued at Canton, no appearance of the motions here treated of, worth remarking, could be perceived; but as soon as the ship left the river, on the 13th of September, they again took place, and continued until the 13th of October; when, upon entering the Strait of Sincapore, they appeared to be diminished, but re-assumed their usual appearance as soon as the ship had passed the narrow part of the strait. On the 21st of October the ship arrived in the harbour of Prince of Wales's Island, and a great diminution again took place in the equatropical motions; but upon

leaving the harbour, on the 5th of November, they again returned, and continued, with their usual regularity, until the arrival of the ship at the entrance of Hoogly river, on the 3rd of December. While the ship continued in the lower part of that river, a slight tendency to the equatropical motions might be perceived; but up the river, at Diamond harbour, the mercury was nearly stationary the whole twenty-four hours.

On the 13th of January 1804, after clearing Hoogly river, the equatropical motions again returned, and continued until the arrival of the ship at Bombay, on the 12th of February; from which day to the 18th, when the journal ceases, no signs of the above motions could be perceived.

Concerning the Differences in the magnetic Needle, on Board the Investigator, arising from an Alteration in the Direction of the Ship's Head. By Matthew Flinders, *Esq. Commander of His Majesty's Ship Investigator. In a Letter to the Right Hon. Sir* Joseph Banks, *K.B. P.R.S.* Read March 28, 1805. [*Phil. Trans.* 1805, *p.* 186.]

In the years 1801 and 1802, while Capt. Flinders, on board the Investigator, was surveying the south coast of New Holland, he observed a difference in the direction of the magnetic needle, for which there appeared no other cause than that of the ship's head being in a different direction. The compasses made use of on board the abovementioned ship were of Walker's construction, one excepted, which was made by Adams; and it appears, from a table of observations given by Capt. Flinders, that some of the variations here treated of were 4° less, and others 4° greater than the truth. It also appears, that when this error was to the west, the ship's head was to the east, or nearly so; when the error was eastward, the ship's head was in a contrary direction; and when the observations agree best with those taken on shore, which may be considered as having the true variation, the ship's head was nearly north or south. A minute inspection of the table seems to favour the opinion, that the excess or diminution of the variation was generally in proportion to the inclination of the ship's head, from the magnetic meridian, on either side.

Capt. Flinders, having ascertained the certainty of a difference in the compass, arising from an alteration in the point steered, thought it necessary, when he wanted a set of bearings from a point where the ship tacked, to take one set just before and another immediately after that operation. Several specimens of the manner in which these bearings were taken are given; also a specimen of the plan he followed in protracting such bearings: these specimens are in the form of tables, and are not of a nature to be abridged.

With respect to the cause of the differences here treated of, Capt. Flinders offers the following conjectures:—

1st, That the attractive power of the different substances in a ship, which are capable of affecting the compass, is brought into a sort of focal point nearly in the centre of the ship, where the shot are de-

posited; the greatest quantity of iron being collected together at that part.

2ndly, That this point is endued with the same kind of attraction as the pole of the hemisphere where the ship is: consequently, in New Holland, the south end of the needle would be attracted by it, and the north end repelled.

3rdly, That the attractive power of this point, in a ship of war, is sufficiently strong to interfere with the action of the magnetic poles, upon a compass placed upon or in the binacle.

The above suppositions, Capt. Flinders thinks, will account for all the observed differences: and, admitting this opinion to be well founded, it ought, he says, to follow, that when the ship is on the north side of the magnetic equator, the differences in the variation of the magnetic needle, arising from a change in the ship's head, must be directly contrary to those above described. A few observations are given, which tend to confirm this opinion, and which also seem to show that the variation is more westerly when taken upon the binacle of a ship whose head is westward in north latitude, than when observed in the centre of the ship.

Capt. Cook having observed a considerable variation in the compass while taking some observations upon Pier Head, on the coast of New Holland, Capt. Flinders thought it right to make some fresh observations at that place. He found, as Capt. Cook had done, that the stones which lay on the surface of the ground did not produce any sensible effect upon the needle, but that a considerable variation took place, by a change of situation of a few yards only, at the top of the hill. Whether this arises from a particular magnetic substance lodged in the heart of the hill, or from the attractive powers of all the substances of which Pier Head is composed being centered in a point, similar to what Capt. Flinders has supposed to happen in a ship, is, he says, a question he shall not attempt to decide.

The Physiology of the Stapes, one of the Bones of the Organ of Hearing; deduced from a comparative View of its Structure and Uses in different Animals. By Anthony Carlisle, *Esq. F.R.S.* Read April 4, 1805. [*Phil. Trans.* 1805, *p.* 198.]

The bones of the organ of hearing, or ossicula auditûs, in man and in the mammalia, form, Mr. Carlisle says, a series of conductors, whose office seems limited to the conveyance of sounds received through the medium of air; no parts corresponding to such bones being found in fishes. In two of the classes of animals, however, namely, birds, and the amphibia of Linnæus, there is only one ossicle of the tympanum, which is in the situation of the stapes.

Mr. Carlisle then proceeds to give a minute description of the human ossicula auditûs, especially of the stapes. This description we shall pass over, that we may be the more particular in our account of the varieties observed in the last-mentioned bone in other animals.

The configuration of the stapes, or indeed of the other ossicles, is

not governed, Mr. Carlisle says, by the form, habits, or voice of the animal, except in those mammalia which inhabit the waters, such as the seal, the walrus, and the whale tribe: in these the stapes is more massive; but in the otter, which only dives occasionally, the stapes does not differ from that of the fox. In the tiger, the dog, and other feræ, the crura are straight, and meet in an acute angle; but the same figure occurs in the horse, in the beaver, in the goat, and in many other herbivorous quadrupeds. In the cete, the muscle of the stapes pulls the capitulum at such an angle, as very much to depress its subjacent end into the fenestra vestibuli; and the joint appears capable of considerable motion. In the walrus, this ossicle is entirely solid: in the seal, and in the cete, the bone has only a small perforation instead of the crural arch.

—Mr. Carlisle has discovered a very remarkable singularity in the stapes of the marmot, and in that of the guinea-pig. In those animals, the bone is formed of slender crura, constituting a rounded arch: through this arch an osseous bolt passes, so as to rivet it to its situation. This bolt, to which Mr. Carlisle has given the name of Pessulus, is placed near the top of the arch, so that, by the action of the stapedeus muscle, the upper part of the straight crus is brought into contact with the pessulus. The use of this mechanism is not obvious, there being nothing in these animals, excepting their shrill whistle, peculiarly different from others which are destitute of such mechanism. In the kanguroo, the stapes is like the corresponding ossicle in birds, called Columella. In the two species of Ornithorhynchus (paradoxus and hystrix), this resemblance to the columella is still more striking, and forms an additional point of similarity between these singular quadrupeds and birds. These columellæ are articulated to a small bone, which performs the office of the manubrium of the malleus; whereas, in birds, the capitulum of the columella is slightly expanded, and is joined to a triangular plate of cartilage attached to the membrana tympani. In some birds, a small foramen occurs in the middle of this plate.

The amphibia are provided with columellæ similar to those of birds; but the cartilage is united to the under surface of the true skin, without any apparent application of muscles to alter its tension.

From the preceding circumstances, Mr. Carlisle is led, he says, to the following conclusions:—In man, and in most of the mammalia, the figure of the stapes is an accommodation to that degree of lightness which seems a requisite condition; and that bone is especially designed to press on the fluid contained in the labyrinth; the ultimate effect of which pressure is, an increase of the tension of the membrane closing the fenestra cochleæ.

There does not, in Mr. Carlisle's opinion, appear to exist any motion between the ossicula auditûs that bears any relation to the peculiar vibration of sounds. He rather conceives, that the different motions of these bones only affect the membrana tympani, so as to lessen the intensity of violent impulses. Sounds of less impetus, not

requiring such modulation, are transmitted by the vibrations of the integrant parts of these bones, unaccompanied by muscular action.

This reasoning, Mr. Carlisle says, is suggested by the columellæ in the aves and amphibia; for, since many birds accurately imitate a variety of sounds, it may be inferred that they hear as acutely and as distinctly as mankind.

The muscles of the ossicula auditûs appear to be of the involuntary kind; their peculiar stimulus is sound, and the chorda tympani is a gangliated nerve. If the above supposition is true, the muscles may be considered as all acting together; especially as it is well known that some persons who hear imperfectly are more sensible to sounds when in a noisy place; as if the muscles were then excited to action.

It cannot, Mr. Carlisle thinks, be allowed, that the pressure of the watery fluid in the labyrinth is necessary to produce the sensation of hearing, since birds hear without any such mechanism: such pressure, however, would give increased tension to the fenestra cochleæ; and, as the membrane of that fenestra is exposed to the air contained within the cavity of the tympanum, it appears adapted to receive such sounds as pass through the membrana tympani, without exciting consonant motions in the ossicula auditûs.

In order to investigate the truth of the above opinions, Mr. Carlisle had water, at the temperature of his body, dropped from a small vial into the meatus externus, the tragus being previously pulled towards the cheek. The first drop produced a sensation like the report of distant cannon; and the same effect succeeded each drop until the cavity was filled.

In this experiment the vibrations of the membrana tympani must, he says, have been impaired, if not destroyed; yet the motions of the membrane produced by each drop of water affected the air contained in the tympanum, sufficiently to produce a sensible impression.

That something like this occurs in many kinds of sounds, is, Mr. Carlisle thinks, more than probable; and as the cochlea consists of two hollow half cones, winding spirally, and uniting at their apices, it follows that the sounds affecting either of the cones must pass from the wide to the narrow end; and the tension of the parts, in either case, will necessarily aid the impression.

On an artificial Substance which possesses the principal characteristic Properties of Tannin. By Charles Hatchett, *Esq. F.R.S.* Read April 25, 1805. [*Phil. Trans.* 1805, *p.* 211.]

Mr. Hatchett, after mentioning the experiments made by several eminent chemists on the substance generally called Tannin (but which he thinks would be better expressed by the word Tan), observes, that the results of those experiments have established, that tan is a peculiar substance, naturally formed, and existing in many vegetable bodies, such as oak bark, &c.; but that no one has ever

supposed it could be produced by art, unless a fact observed by Mr. Chenevix, namely, that a decoction of coffee-berries did not precipitate gelatine until they had been roasted, may be considered as an exception. Some recent experiments, however, have, he says, convinced him, that tan may be formed, not only from vegetables, but even from mineral and animal substances.

The powerful effects observed by Mr. Hatchett to be produced on resinous substances by nitric acid, and the discovery made by him of a natural substance, composed partly of resin and partly of asphaltum, induced him to extend his experiments to the bitumens. He had found, that almost every species of resin is completely dissolved in nitric acid, and so totally changed that water does not cause any separation; and that, by evaporation, a deep yellow viscid substance is obtained, equally soluble in water or in alcohol: whereas, the first effect of the above acid on some of the bitumens, for instance, asphaltum and jet, was to form a dark brown solution, whilst an orange-coloured mass was separated, which, by subsequent digestion in another portion of nitric acid, was completely dissolved, and, by evaporation, afforded a yellow viscid substance, nearly similar to that obtained from the resins. But coals, which contained little or no bitumen, did not yield the yellow substance above mentioned.

Mr. Hatchett then made a similar experiment upon charcoal, and found it was more readily dissolved than the preceding substances; no residuum was left; and the solution was of a reddish brown colour.

All the above solutions, when carefully evaporated to dryness, left a brown glossy residuum, which exhibited a resinous fracture; that left by the solution of charcoal having the most beautiful appearance.

The chemical properties of these residua were as follows:—

1. They were speedily dissolved by cold water and by alcohol.
2. Their flavour was highly astringent.
3. When exposed to heat, they smoked but little, swelled much, and afforded a very bulky coal.
4. Their solutions in water reddened litmus-paper.
5. These solutions copiously precipitated the metallic salts, especially muriate of tin, acetite of lead, and oxysulphate of iron.
6. They precipitated gold from its solution in the metallic state.
7. They also precipitated the earthy salts, such as the nitrates of lime, of barytes, &c.
8. The fixed alkalies, as well as ammonia, deepen the colour of these solutions, and, after some hours, render them turbid.
9. Glue or isinglass was immediately precipitated from water by these solutions. These precipitates were, in every respect, similar to those formed by the solutions of tan hitherto known, excepting that this factitious tan appeared to be exempt from the extract, gallic acid, and mucilage, which commonly accompany natural tan.

Mr. Hatchett, having thus discovered that tan might be so readily formed from vegetable and mineral coals, was led to examine whether it could not also be formed from animal coal. For this purpose, he

reduced a portion of isinglass to the state of coal, and digested it in nitric acid, which at first did not appear to act upon it, but at length slowly dissolved almost the whole of it. The solution resembled those which have been described, but was of a deeper brown colour; and, when evaporated to dryness, left a residuum, which, upon being examined by the re-agents employed in the former experiments, was found to produce similar effects.

It appears evident, therefore, that tan may be formed from animal as well as from vegetable and mineral coal; and it also appears, from what has been stated, that it is composed of carbon, combined with a certain proportion of oxygen. It seems, however, necessary that the carbon should be uncombined with any other substance. In support of this opinion, Mr. Hatchett mentions the following experiments:—

1. A piece of Bovey coal, which appeared like half-charred wood, upon being treated with nitric acid, formed a solution of a deep yellow colour: this solution, when evaporated, left a residuum, which, dissolved in distilled water, and examined by various re-agents, particularly by gelatine, did not show any signs of its containing tan; the predominant substance appearing to be oxalic acid.

2. Another piece of Bovey coal, which was more perfectly carbonized, afforded a brown solution, which, unlike the former, yielded a considerable quantity of tan.

3. A portion of the first-mentioned sort of Bovey coal, by being exposed to a red heat in a close vessel, and then treated as before, was thus converted, almost entirely, into tan.

4. A coal from Sussex, very like the second sort of Bovey coal, also afforded tan.

5. A piece of Surturbrand, from Iceland, yielded a similar result.

6. Deal sawdust, treated in the same manner as the former substances, afforded oxalic acid, but not any tan.

7. Another portion of the same sawdust was reduced into charcoal, which, treated as before, was thereby converted into tan.

8. Teak wood, which Mr. Hatchett had previously ascertained not to contain either gallic acid or tan, was reduced into charcoal, which was as readily converted into tan as the substances already mentioned.

Mr. Hatchett then adverts to a series of experiments he is making on the slow carbonization of vegetable substances in the humid way, a few of which, he says, he is compelled to notice, on account of their being intimately connected with the present subject. In these experiments he has observed, that concentrated sulphuric acid dissolves resinous substances, forming a yellowish brown transparent solution, which, by digestion, becomes intensely black. Concentrated sulphuric acid readily dissolves the common turpentine of the shops. If a portion of this solution be immediately poured into cold water, the turpentine is precipitated, in the state of common yellow resin. But if another portion of the same solution be, after the lapse of an hour or more, poured into cold water, the resin thus formed is not yellow,

but dark brown. If four or five hours elapse before the solution is poured into the water, the resin precipitated is found to be completely black. And if the digestion is continued for several days, or until there is no longer any production of sulphureous gas, the turpentine is converted into a black porous coal, which does not contain any resin, although a substance hereafter noticed may frequently be separated from it by digestion in alcohol.

When common resin was treated in the same manner, about 43 per cent. of the coal was obtained, which, after exposure to a red heat in a loosely-covered platina crucible, still amounted to more than 30 per cent., and appeared to possess properties very similar to those of some of the mineral coals.

Mr. Hatchett having obtained, in the manner above described, yellow resin, brown resin, black resin, and coal, from a quantity of common turpentine, dissolved a portion of each of these, and also of the turpentine, in nitric acid, and then reduced the solutions to dryness. The residua, which varied in colour, from yellow to dark brown, were dissolved in distilled water, and examined by solution of isinglass and other re-agents.

1. The solution of the residuum of turpentine was of a pale straw colour, and did not contain any tan.

2. That of the yellow resin resembled the former in every respect.

3. That of the brown resin was of a deeper yellow, but did not afford a vestige of tan.

4. That of the black resin, on the contrary, afforded a considerable portion of tan.

5. That of the coal afforded tan in great abundance.

Hence it appears, that these modifications of turpentine yield tan only in proportion to the quantity of their original carbon, progressively converted into coal.

Other substances, particularly various kinds of wood, copal, amber, and wax, when reduced into coal in the humid way, were in like manner converted into tan by nitric acid.

But tan may, Mr. Hatchett says, be artificially produced, without the help of nitric acid; for if any of the resins, or gum resins, after long digestion with sulphuric acid, are digested with alcohol, a dark brown solution is formed, which, by evaporation, yields a mass that is soluble in water or in alcohol, and which copiously precipitates gelatine, acetate of lead, and muriate of tin, but produces only a slight effect on oxymuriate of iron.

In the subsequent section of this paper, Mr. Hatchett mentions some circumstances which induce him to think that a natural process, similar to those above described, sometimes takes place in peat moors, and that tan has been, and continues to be, formed during the gradual carbonization and conversion of the vegetable matter into peat. Supposing this opinion to be correct, it seems, he says, at first difficult to conceive how the formation of tan is effected during the growth of those vegetables from which it has hitherto been obtained; but after adverting to the experiments and observa-

tions of Mr. Biggin and Mr. Davy, which show that the proportion of tan in the same trees is different at different seasons, and that it is principally contained in the white interior bark, which bark is comparatively most abundant in young trees, he observes, that there seems to be an intimate connexion between the formation of new wood and the formation of tan, in those vegetables which afford the latter; and thinks it very probable that such vegetables have the faculty of absorbing more carbon and oxygen than is required in the formation of the vegetable principles, especially of the woody fibre; and that this excess of carbon and oxygen, by chemical combination, becomes tan, which is secreted in the white interior bark, and afterwards decomposed, and employed in the formation of the new wood.

The ligneous substance of vegetables, Mr. Hatchett says, appears to be composed of carbon, oxygen, hydrogen, and nitrogen; and he has reason to think, from some synthetical experiments he has made, that tan consists of 53 parts of pure carbon, and 47 of oxygen.

In the concluding section, Mr. Hatchett observes, that the whole of the present paper may be concentrated into one simple fact, namely, that tan is composed (at least essentially) of carbon and oxygen; and that, although it has hitherto been deemed a peculiar principle, formed by nature in certain vegetables, it may, with the greatest ease, be produced, by presenting oxygen to carbon in the humid way, under the circumstances which have been described.

Since the experiments which have been related were made, Mr. Hatchett has, he says, further proved the efficacy of the factitious tan by actual practice; as he has converted skins into leather by means of tan produced from materials which, to professional men, must appear extraordinary, such as deal sawdust, asphaltum, turpentine, pit-coal, wax candle, and a piece of the same sort of skin. Allowing, therefore, that the artificial production of tan must for the present be principally regarded only as a curious chemical fact, not altogether unimportant, yet, as the principle on which it is founded has been developed, we may, Mr. Hatchett thinks, hope that a more economical process will be discovered, so that every tanner may be enabled to prepare his tan, even from the refuse of his present materials.

The Case of a full-grown Woman in whom the Ovaria were deficient. By Mr. Charles Pears, *F.L.S. Communicated by the Right Hon. Sir* Joseph Banks, *K.B. P.R.S.* Read May 9, 1805. [*Phil. Trans.* 1805, *p.* 225.]

The woman whose case is here described was born in Radnorshire in the year 1770. She was of a fair complexion, and, except when irritated, of a mild temper. In her diet she was remarkably abstemious, eating very little, either of animal or vegetable food; and if at any time she ate a hearty meal, or took several kinds of food, she was so much affected by it as to faint. She was of a costive habit,

seldom having a passage oftener than once in nine days, sometimes only once in fourteen days. She ceased to grow at ten years of age, and was only four feet six inches in height. Across the shoulders she measured fourteen inches, but her pelvis measured only nine inches, from the ossa ilia to the sacrum. Her breasts and nipples never enlarged more than those of a man; nor did she ever menstruate, or show any other sign of puberty, either in mind or body; on the contrary, she always expressed aversion to the familiarities of young men.

At the age of twenty-one she became uneasy at finding herself different from other women, and, attributing the difference to her not having menstruated, frequently applied for medical advice.

She was, from her infancy, subject to the attacks of a complaint in the chest, attended with cough. These attacks increased in violence as she advanced in age; and in her twenty-ninth year, one of them came on, attended with convulsions, of which, after a few hours, she died.

Upon examining the female organs after her death, it appeared that the os tincæ and uterus had the usual form, but had not increased beyond their size in the infant state. The passage into the uterus, through the cervix, was oblique, and the Fallopian tubes were pervious to the fimbriæ. The ovaria were so indistinct that they rather showed the rudiments which ought to have formed them, than any part of their natural structure.

From the history of the preceding case, it appears, not only that an imperfect state of the ovaria is attended with an absence of all the characters belonging to the female after puberty, but also that the uterus itself, although perfectly formed, was checked in its growth, in consequence of the imperfect structure of those parts.

A Description of Malformation in the Heart of an Infant. By Mr. Hugh Chudleigh Standert. *Communicated by* Anthony Carlisle, *Esq. F.R.S.* Read May 9, 1805. [*Phil. Trans.* 1805, *p.* 228.]

The infant here treated of died at the age of ten days, during which period nothing particular was remarked, except that the skin exhibited the blue colour so common in cases of imperfect pulmonary circulation.

Upon opening the body, all the viscera were found in the natural state, except the heart, which exhibited the following remarkable structure:

Externally, only one auricle could be perceived, into which the pulmonary veins and venæ cavæ entered in the usual manner. The pulmonary artery was wanting, and the body of the heart had but one ventricle, which was separated from the auricle by tendinous valves, and opened into the aorta.

The auricle was also single, and had a narrow muscular band, which crossed the ostium venosum, in the place of the septum. The aorta sent off an artery from the situation of the ductus arteriosus: this

artery was divided into two branches, to supply the lungs. These vessels were of small diameter.

The pulmonary veins were four in number; but the area of these, and that of the vessel which acted as the pulmonary artery, did not exceed half the usual dimensions.

The child, while alive, was seen by Dr. Combe, who did not observe that its respiration, temperature, or muscular action, were materially affected.

On a Method of analysing Stones containing fixed Alkali, by Means of the Boracic Acid. By Humphry Davy, *Esq. F.R.S. Professor of Chemistry in the Royal Institution.* Read May 16, 1805. [*Phil. Trans.* 1805, *p.* 231.]

The method of analysis here described by Mr. Davy is founded on the attraction of the boracic acid for the simple earths, which is considerable at the heat of ignition, and on the ease with which the compounds formed with them are decomposed by the mineral acids.

The process is as follows: 100 grains of the stone to be examined must be fused for about half an hour, in a strong red heat, with 200 grains of boracic acid: an ounce and a half of nitric acid, diluted with seven or eight times as much water, must be digested upon the mass till the whole is decomposed; and the fluid must then be reduced, by evaporation, to an ounce and a half or two ounces.

If the stone contain silex, it will now be separated: this must be collected upon a filter, and washed with distilled water till freed from the boracic acid and all other saline matter.

The water that has passed must be mixed with the other fluid, and the mixture evaporated till it is reduced to a convenient quantity, for instance, half a pint. It must then be saturated with carbonate of ammonia, and boiled with an excess of this salt till all precipitable matter has fallen to the bottom of the vessel.

The earths and metallic oxides must be separated by the filter, and to the filtered liquor must be added nitric acid, till it tastes very sour: it must then be evaporated till the boracic acid appears free.

The fluid must be again passed through the filter, and evaporated to dryness; when, by exposure to a degree of heat equal to 450° of Fahrenheit, the nitrate of ammonia will be decomposed, and the nitrate of fixed alkali will remain in the vessel.

The remaining earths and oxides Mr. Davy has separated by the usual processes. The alumina he has separated by solution of potash; the lime by sulphuric acid; the oxide of iron by succinate of ammonia; the manganese by hydrosulphuret of potash; and the magnesia by pure soda.

On the Direction and Velocity of the Motion of the Sun, and Solar System. By William Herschel, *LL.D. F.R.S.* Read May 16, 1805. [*Phil. Trans.* 1805, *p.* 233.]

Although, in the title to this paper, Dr. Herschel mentions both the direction and velocity of the solar system, it is his intention, he says, to limit his inquiries, at present, to the first of these subjects, and to discuss the other at some future opportunity. He is induced to enter into this inquiry, because a solar motion, if established, seems to contradict the original intention for which it was introduced; namely, to take away many of the proper motions of stars, by investing the sun with a contrary one. But as the solar motion will reveal a greater number of concealed real motions than need be admitted if the sun were at rest, the necessity of admitting its motion ought to be well established.

From the motion of the secondary planets round the primary ones, and of these round the sun, the solar motion must be allowed to be a very possible event; and the rotatory motion of the sun, from which a displacing of the solar centre has been inferred, also indicates a motion of translation in space; for it does not appear probable that any mechanical impression should produce the former without occasioning the latter.

It would, Dr. Herschel thinks, be worth while for those who have fixed instruments, to observe those stars which change their magnitudes periodically; for, as this change is probably owing to a rotatory motion, a real motion in space may be expected to attend it: and, on the other hand, all those stars that have a motion in space may be supposed to have also a rotation on their axes.

Dr. Herschel now proceeds to consider the symptoms of parallactic motions. If, says he, the sun be supposed to move towards a certain part of the heavens, the stars will appear, to an inhabitant of the earth, to move in an opposite direction. This may be called the parallactic motion of a star; and, if the star has no real motion, it will also be its apparent motion; but, if the star should have a real motion, it will appear to move along the diagonal of a parallelogram, which diagonal will represent its real motion. This is illustrated by a diagram, to which we must refer for a fuller explanation of this part of the paper. We shall only observe, that the absolute motion of a star in space will still remain unknown, as well as its velocity, because the inclination of that motion, on which its real velocity will depend, admits the greatest variety of directions.

In order to ascertain whether parallactic motions exist, we ought, Dr. Herschel says, to examine the brightest stars; it being probable that they are most liable to be visibly affected by solar motion: and we should also seek for a criterion by which parallactic motions may be distinguished from real motions. This we find in their directions; for, if a solar motion exists, all parallactic motions will tend to a point in opposition to its direction; whereas real motions will be dispersed indiscriminately to all parts of space.

Dr. Herschel has delineated the meeting of the arches, arising from a calculation of the proper motions of the 36 stars in Dr. Maskelyne's Catalogue, on a celestial globe; and finds that, in the northern hemisphere, no less than ten of those intersections are made by stars of the first magnitude, in a very limited part of the heavens, about the constellation of Hercules. Upon all the remaining surface there is not the least appearance of any other than a promiscuous situation of intersections, and only one of these made by arches of principal stars.

A table is then given of the calculated situations of the above-mentioned ten intersections in right ascension and north polar distance; and it is observed, that if the intersections made by the proper motions of some large stars of the next order, and the arches in which the stars of the first magnitude move, are examined, no less than fifteen unite with the former ten in pointing out the same part of the heavens as a parallactic centre. This, Dr. Herschel thinks, can hardly fail to be considered as a convincing proof of the motion here treated of.

The changes in the position of double stars are next considered; and these, Dr. Herschel thinks, it will be more eligible to ascribe to the effect of parallax than to admit so many separate motions in the different stars, especially as the parallactic motions of at least half of the 56 double stars described by him, point out the same apex of a solar motion by their direction to its opposite parallactic centre.

Dr. Herschel then remarks, that if the proper motions of the stars were such as they appear to be, they would exhibit an incongruous mixture of great velocity and extreme slowness. Of this incongruity, several instances are enumerated; but it will, he says, be shown, when the direction and velocity of the solar motion are explained, that these incongruities are mere parallactic appearances.

With respect to the occultation of a small star by a large one, Dr. Herschel will, he says, prove, when the solar motion is established, that the vanishing of the small star near δ Cygni is, as far as we can judge at present, only a parallactic appearance.

Dr. Herschel now proceeds to consider the direction of the solar motion; the expedience of admitting such motion being, he thinks, after what has been said, no longer questionable.

He begins by proving, that when the proper motions of two stars are given, an apex may be found, to which, if the sun be supposed to move with a certain velocity, the two given motions may be resolved into apparent changes, arising from sidereal parallax; the stars remaining perfectly at rest. The mode of proving this, in which Arcturus and Sirius are used as examples, will not admit of abridgement. But, from the nature of proper motions, it follows, that when a third star does not lead us to the same apex as the other two, its apparent motion cannot be resolved by the effect of parallax alone: and, although we may account for the proper motion of the third star, Capella for instance, by retaining the same apex of the solar motion which explained the apparent motions of the other

two, yet, in doing this, we must assign a high degree of real motion to Capella. To this it may be objected, that we have no reason to deprive Arcturus and Sirius of real motions, in order to give a motion of the same nature to every star that has a proper motion not tending to the same parallactic centre as the motions of Arcturus and Sirius.

It appears, therefore, that such an apex for the solar motion ought to be fixed upon as is equally favourable to every star that is proper for directing our choice; and our aim must be, to reduce the proper motions of the stars to their lowest quantities.

From a table given by Dr. Herschel, it appears, that the sum of the apparent motions of the six principal stars whose intersecting arches are given, namely, Sirius, Arcturus, Capella, Lyra, Aldebaran, and Procyon, is $5''\cdot353$; and if we suppose the point towards which the sun moves to be λ Herculis, the annual proper motions of the six stars will be reduced to real motions of no more than $2''\cdot219$.

It appears, from the inspection of a figure that represents the quantities of real motion required when λ Herculis is fixed upon, that, by a regular method of approximation, a situation might be found where the apparent motion of the six stars would be much reduced. Accordingly, by fixing upon a point near the following knee of Hercules, whose right ascension is $270^\circ\ 15'$, and north polar distance $54^\circ\ 45'$, the annual proper motion of the six stars was reduced to $1''\cdot459$, which is $0''\cdot760$ less than when the apex was λ Herculis.

In approximating to the above point, the line of the apparent motion of Sirius was principally considered; but, as Sirius is not the star that has the greatest proper motion, it occurred to Dr. Herschel that another minimum, obtained from the line in which Arcturus seems to move, would be more accurate; and he was soon led to a point, not only in the line of the apparent motion of Arcturus, but equally favourable to Sirius and Procyon, the remaining two stars that have the greatest motion. The right ascension of this point is $245^\circ\ 52'\ 30''$, and its north polar distance $40^\circ\ 22'$.

If the principles which have been laid down for determining the solar motion are admitted, the above apex must be very near the truth; for an alteration of a few minutes in right ascension or polar distance, either way, will increase the required real motion of these stars. The sum of the real motions with the before-mentioned apex is only $\cdot859$, being less than that of the former calculation by $\cdot599$.

Dr. Herschel does not, he says, mean to assert that these real motions can be actually reduced to the low quantities above mentioned; but, whatever may be the sum of real motions required to account for the phenomena of proper motions, the foregoing arguments cannot be affected by the result; for, as it is known that proper motions exist, and no solar motion can resolve them entirely into parallactic motions, we ought to give the preference to that direction of the motion of the sun that will take away more real motion than any other.

On the Reproduction of Buds. By Thomas Andrew Knight, *Esq. F.R.S. In a Letter to the Right Hon. Sir* Joseph Banks, *K.B. P.R.S.* Read May 23, 1805. [*Phil. Trans.* 1805, *p.* 257.]

Mr. Knight begins his paper by stating, that every tree, in the usual course of its growth, generates the buds that expand in the succeeding spring; but if these buds are destroyed, during the winter or early part of the spring, other buds are in many species generated; which buds perform the office of those that previously existed, except that they never afford blossoms or fruit. This reproduction of buds has not escaped the notice of naturalists; but it does not appear that they have ascertained from which of the various substances of the tree the reproduced buds derived their origin.

After noticing some erroneous opinions respecting the origin of buds, Mr. Knight proceeds to relate some observations and experiments made by him on this subject. If the fruit-stalks of the Sea Cale (*Crambe maritima*) are cut off in the spring, the medullary substance decays, and a cup is formed, the sides of which consist of a woody substance, perfectly similar to the alburnum of trees. From the interior part of this substance, new buds are frequently generated in the ensuing spring: hence it is obvious, that the buds, in this case, do not spring from the bark; but it is not equally evident that they do not spring from some remains of the medulla.

In the autumn of 1802, Mr. Knight discovered that the potatoe possessed a similar power of reproducing its buds; which buds sprung from tubers generated on the surfaces made by the knife in dividing the root. In a former paper he has given some reasons for supposing that the internal substance of the potatoe is alburnous: he now observes, that there is in the young tuber a transparent line through the centre, which is probably its medulla; and that the reproduced buds did not spring from the central part, nor from the surface in contact with the bark, but from what he has supposed to be the alburnum of the root.

The author now gives an account of the experiments made, in the autumn of 1802, on young apple, pear, and plum trees, raised by him from seed, and, at that time, about two inches above the ground. These plants, after removing some of the soil, were cut off, about an inch below where the seed-leaves formerly grew; so that a portion of the root, about an inch long, and without any bud upon it, remained exposed. In the beginning of April, many small elevated points were seen on the bark; these appeared to proceed from the alburnum, and, as the spring advanced, perforated the bark, and produced shoots.

As it might be supposed that in the preceding experiments the buds may have originated from the medulla, Mr. Knight thought it right to make some similar experiments on old trees; and found the buds were reproduced by such trees, exactly in the same manner as by the annual roots.

Mr. Knight, in a former paper, has remarked, that the central

vessels appear to derive their origin from the alburnous tubes; he now thinks it not improbable that the lateral, as well as the terminal orifices of the alburnous tubes, may possess the power of generating central vessels, and that these vessels give existence to the reproduced buds and leaves.

Mr. Knight attempted to discover in seeds a similar power to regenerate their buds; but no experiments he could make were decisive, as he was never able to satisfy himself that all the buds could be eradicated without the base of the plumula being destroyed.

The power of reproducing buds here treated of, is not possessed, Mr. Knight says, by annual or biennial plants; but he relates that a turnip, from which the greater part of its fruit-stalks had been cut off, and of which all the buds had been destroyed, remained some weeks in an apparently dormant state; the first seed in each pod then germinated, and, bursting the seed-vessel, seemed to perform the office of a bud and leaves to the parent plant during the short remaining term of its existence.

Mr. Knight takes this opportunity to correct an inference drawn by him, in a former paper, from an experiment in which, after inverting a shoot of a vine and removing a portion of its bark, more new wood was generated on the lower lip of the wound, now become uppermost, than on the opposite lip. He has there inferred, that this effect was produced by sap which had descended from the leaves above. But as the branch was employed as a layer, the matter which would have accumulated on the opposite lip of the wound had been expended in the formation of roots; a circumstance which, at that time, escaped Mr. Knight's attention.

Some Account of two Mummies of the Egyptian Ibis, one of which was in a remarkably perfect State. By John Pearson, *Esq. F.R.S.* Read June 13, 1805. [*Phil. Trans.* 1805, *p.* 264.]

After some general observations on the art of embalming, as it was practised by the ancient Egyptians, and on the various kinds of animals embalmed by them, Mr. Pearson proceeds to give a particular description of the very perfect mummy of an Ibis, which forms the chief subject of the present paper.

This mummy was taken out of the catacombs at Thebes, by the late Major Hayes, in the year 1802 or 1803. It was enveloped in cloth, and contained in an earthen jar, similar to those which are found at Saccara. Upon unrolling the bandage with which the mummy was covered, it was found to consist of strips of cloth, about three inches broad, which were strong and firm. The first circumvolutions of this cloth separated easily; but as the work proceeded, they were found to adhere more firmly, and at last were so closely united, that it was necessary to divide them by means of a strong knife. Each layer of cloth seemed to have been imbued with some bituminous substance in a liquid state; and the bandages were further secured by means of thread, in such a manner that the whole mass

was rendered firm and coherent; when the bandage was removed, the bird appeared to be covered with the same kind of bituminous substance that had cemented the strips of cloth. As much of this substance as could be removed without injuring the bird was now carefully taken off; and after the labour of several hours, Mr. Pearson succeeded in displaying the whole bird as it had been originally deposited by the embalmer.

The neck of the bird was twisted, so that the vertex of the head lay a little to the left of the sternum. The bill descended between the feet, and reached to the extremity of the tail. The feet were bent upwards, and placed one on each side of the head. The wings were brought close to the sides of the body. The feathers of the back and wings were white, tipped at their extremities with dark brown. The tail feathers could not be sufficiently cleared from the bituminous substance, to determine their colour. From the state of the quills of the wing feathers, it appeared that the bird had attained its full growth.

The dimensions of this bird were as follows:

	inches.
From the termination of the neck to the extremity of the tail	$12\frac{1}{2}$
Length of the neck	$6\frac{1}{2}$
——— head and bill	8
——— sternum	4
——— metatarsal bone	$3\frac{1}{2}$
Longest toe	$3\frac{1}{4}$
Width at the shoulders	$4\frac{1}{2}$
Circumference of the body	$13\frac{1}{2}$

Weight of the whole, $16\frac{1}{2}$ ounces, Troy.

No particular marks of decay can be perceived in this mummy, although it is probable, Mr. Pearson says, that the greater part of 3000 years has elapsed since it was embalmed. It was, he thinks, immersed in the bituminous matter, while that matter was in a liquid state; but that it was not boiled therein, as Grew supposed, is evident, from the feathers not being corrugated, or otherwise materially changed from their natural state.

Mr. Pearson unrolled another mummy of an Ibis, also sent from Thebes by Major Hayes, which appeared to have been embalmed in a different manner. The cloth of this latter was of a coarser texture; it had not been so thoroughly imbued with bitumen, nor were the circular bands continued to the body of the bird, which was merely wrapped in several pieces of cloth. This mummy was in such a state of decay, that no remains of the head or bill could be discovered. The exterior layer of feathers was in general of a dark colour, some of them tipped with white. The plumage of the neck and tail was white; the latter had a tufted appearance.

Whether the two birds here described were what authors have called the white and the black Ibis, Mr. Pearson cannot, he says, presume to determine. With respect to the mode in which such birds were embalmed, it appears, he thinks, contrary to what is

stated by Herodotus to have been the practice in embalming human bodies, that the stomach and intestines were not removed, as upon examining the interior parts of the last-mentioned Ibis, Mr. Pearson met with a soft spongy substance, containing several scarabæi in an imperfect state. These, he supposes, had been taken as the food of the bird, and were not digested at the time of its death. He also observes, that as larvæ of dermestides and other insects have been detected among the dust and bones of the mummy of an Ibis, it may be presumed that this bird was not always in a fresh state at the time when it was embalmed.

Observations on the singular Figure of the Planet Saturn. By William Herschel, *LL.D. F.R.S.* Read June 20, 1805. [*Phil. Trans.* 1805, *p.* 272.]

Notwithstanding the variety of extraordinary phenomena already observed respecting the planet Saturn, there remains, Dr. Herschel says, a singularity which distinguishes the figure of Saturn from that of all the other planets.

He had, in the year 1776, observed that the body of Saturn was not exactly round, and had found in the year 1781 that it was flattened at the poles, at least as much as Jupiter. In the year 1789 he measured the equatorial and polar diameters, and supposing there could be no other particularity in the figure of the planet, ascribed a certain irregularity he perceived in other parts of the body, to the interference of the ring.

Dr. Herschel now relates a series of observations made in the months of April, May, and June, of the present year, of which the following are the most remarkable.

April 12.—The flattening of the polar regions appeared not so gradual as in Jupiter, and seemed not to begin till at a high latitude.

April 18.—The situation of the four points of the greatest curvature was measured with Dr. Herschel's angular micrometer, power 527. Their latitude was found to be 46° 38′; but as neither of the cross wires could be in the parallel, no great accuracy, Dr. Herschel says, could be expected.

April 19.—Ten-feet reflector, power 400. The figure of Saturn was somewhat like a parallelogram, with the four corners rounded off deeply. A measure of the position of the four points of the greatest curvature, taken this night, gave their latitude 45° 44′·5.

May the 5th, 12th, and 13th.—Ten-feet reflector, with different powers. Jupiter and Saturn were viewed alternately, and compared. A greater curvature was evident at the polar and equatorial regions of Jupiter than at those regions in Saturn. These alternate observations were many times repeated, and the oftener the planets were compared, the more striking appeared the difference in their shape.

May 26.—Ten-feet reflector, power 400. The difference in the three diameters of Saturn was evident without measurement. That which passes through the points of the greatest curvature being the

largest; the equatorial diameter the next; and the polar diameter the smallest.

June 1.—Two measures of the latitude of the greatest curvature were taken, by setting the fixed thread of the micrometer to the direction of the ring. The mean of these was 43° 20′.

June 2nd.—The two planets were viewed alternately, with powers of 300, of 200, and of 160; and even with the lowest of these, the difference in the figure of the planets was very evident.

The telescopic appearance of Saturn was then compared with a figure drawn from the measures Dr. Herschel had taken, combined with the proportion between the equatorial and polar diameters determined in the year 1789. From these a corrected figure was formed, of which an exact copy is given. The dimensions of it, in proportional parts, are,—

Diameter of the greatest curvature	36
Equatorial diameter	35
Polar diameter	32
Latitude of the longest diameter	43° 20′

These observations, Dr. Herschel thinks, may tend, in some measure, to ascertain the quantity of matter in the ring and its solidity; they also afford a new instance of the effect of gravitation on the figure of planets; for in the present case the opposite influence of two centripetal, and two centrifugal forces, must be considered.

On the magnetic Attraction of Oxides of Iron. By Timothy Lane, *Esq. F.R.S.* Read June 20, 1805. [*Phil. Trans.* 1805, *p.* 281.]

Mr. Lane having observed that hardened iron is not so readily attracted by the magnet as soft iron, was proceeding to make some experiments on the subject, when he was led, by Mr. Hatchett's paper on Magnetical Pyrites, &c., to examine what magnetical properties iron possessed when free from inflammable matter. For this purpose he procured some of the precipitate sold at Apothecaries' Hall under the name of ***Ferrum precipitatum***, and which is prepared by adding purified kali to a solution of sulphate of iron. This precipitate, the author says, has no magnetic particles; nor, when exposed to a clear red heat, does it acquire any, except when smoke or flame have access to it. The solar heat, when concentrated to the degree at which glass melts, does not render this oxide magnetic, provided it be protected by glass from the dust floating in the air; if not so protected, many of the particles become magnetic.

Mr. Lane then rubbed various portions of the oxide, in a glass mortar, with different combustible substances, namely, coal, sulphur, charcoal, camphor, ether, alcohol, &c., but found the oxide was not thereby rendered magnetic, without the assistance of a degree of heat equal to that of melting lead; with that degree, however, it became magnetic. Hydrogen, when aided by a red heat, had the same effect. Charcoal and cinders, well burnt, were found to require a longer continuance of the heat, to have their full effect on

the oxide, than dry wood, coal, or sulphur. A single grain of camphor, dissolved in an adequate portion of alcohol, was found sufficient, when assisted by a red heat, to render all the particles of 100 grains of the oxide magnetic. But such substances as are easily sublimed, will, by a continued application even of a low heat, quit the oxide, leaving it, as at first, unmagnetic. Hence we may understand why Prussian blue, sulphurets, and ores of iron, containing inflammable matter, become magnetic by the agency of heat, and revert to their unmagnetic state if the heat is continued long enough to drive off the inflammable matter.

The intention of this paper, Mr. Lane says, is to prove that mere oxides of iron are not magnetic; that inflammable substances do not render them magnetic until such substances are, by heat, chemically combined with them; and that when the combustible substance is again separated by heat, the oxides return to their unmagnetic state.

Additional Experiments and Remarks on an artificial Substance, which possesses the principal characteristic Properties of Tannin. *By* Charles Hatchett, *Esq. F.R.S.* Read June 27, 1805. [*Phil. Trans.* 1805, *p.* 285.]

Mr. Hatchett was, he says, at first inclined to consider the artificial tanning product as exactly similar to the natural vegetable principle called tannin; but as there appeared to be a considerable difference between them with respect to the effect of nitric acid (which acid produces the one while it destroys the other), he thought it necessary to make some experiments on the comparative effects of this acid on those substances which contain the largest proportions of tannin.

He accordingly subjected the artificial product, sometimes alone, and sometimes mixed with other substances, to the action of nitric acid; and although Mr. Hatchett cannot, he says, assert that this substance is absolutely indestructible when repeatedly distilled with that acid, yet the results of his experiments showed that the destruction of it, by that means, would be a work of considerable time and difficulty.

Muriatic acid also, appeared to have no effect on this substance; and Mr. Hatchett remarks, that the solutions of it seem to be completely imputrescible, also, that they do not become mouldy, like the infusions of galls, sumach, &c.

Some comparative experiments were then made, by means of nitric acid, on galls, sumach, Pegu cutch, kascutti, common cutch, and oak-bark; from which it appeared, that although the artificial product is by much the most indestructible of all the tanning substances, yet there is some difference in that respect between the various kinds of natural tannin; common cutch and the tannin of oak-bark resisting the effects of nitric acid much more than galls, sumach, kascutti, and Pegu cutch.

A number of miscellaneous experiments on the substance here

treated of are then described, of which we shall only mention one, made to show the effects of heat upon it.

Twenty grains of the substance, prepared by means of nitric acid from pure vegetable charcoal, were subjected to a high degree of heat in a glass retort. A small quantity of nitric acid, from which the substance had not been entirely freed, first came over; after this a considerable quantity of ammonia was suddenly produced, which was succeeded by carbonic acid and a small quantity of nitrogen gas. A bulky coal, weighing $8\frac{1}{2}$ grains, remained, which, by incineration, yielded $1\frac{1}{2}$ grain of brownish white ashes, consisting principally of lime.

Mr. Hatchett now relates an experiment made with indigo, by which he ascertained, that a variety of the vegetable tanning substance might be formed without previously converting the vegetable body into coal; and he has, he says, since discovered, that although indigo yields this substance more readily than most other vegetable bodies, yet very few of those bodies are to be considered as exceptions. He had in his former paper stated, that in his experiments upon common resin it was necessary to develope part of the carbon in the state of coal by sulphuric acid, in order to produce the tanning substance; but he has since found, that when the abstraction of nitric acid was several times repeated, that substance might be obtained not only from common resin, but also from several other resinous bodies.

In the course of these experiments Mr. Hatchett found, that by treating dragon's blood with nitric acid, a feather-like sublimate was produced, which had the aspect, odour, and properties of benzoic acid, although no vestige of this acid could be obtained by simply exposing the dragon's blood to heat. Guaiacum, although similar in its general character to resins, when treated as above, yielded only slight vestiges of the tanning product, but, like the gums, afforded a large quantity of oxalic acid.

In the following section Mr. Hatchett observes, that the decoctions of several roasted vegetable substances did not afford any precipitate with a solution of isinglass; even a decoction of coffee did not afford a precipitate until several hours had elapsed; but by adding a small quantity of nitric acid to any of the above decoctions, the tanning substance might be procured from them.

Lastly, the author describes some experiments made by him in order to procure the tanning substance from camphor. For this purpose 100 grains of camphor were dissolved in sulphuric acid, and, after four days, at which time the production of sulphureous acid gas had nearly ceased, cold water was added, and the whole was subjected to distillation; by this, about 3 grains of an essential oil were obtained; and as by a second distillation with water no more essential oil came over, the residuum was treated with successive portions of alcohol until that solvent ceased to act upon it. The residuum had now the appearance of a compact coal, and after desiccation weighed 53 grains. The solution formed by alcohol, upon being

distilled, left a blackish brown substance, weighing 49 grains. This substance appeared, by experiments made upon it, to be a variety of the artificial tanning matter, much resembling that obtained from resinous bodies by means of sulphuric acid; but it is remarkable, that when a small quantity of nitric acid was added to an aqueous solution of the substance obtained from camphor, and, after evaporation to dryness, the residuum was dissolved in water, a reddish brown liquid was formed, which acted in a manner exactly similar to the tanning substance obtained from carbonaceous substances by nitric acid.

On the Discovery of Palladium; with Observations on other Substances found with Platina. By William Hyde Wollaston, *M.D. Sec. R.S.* Read July 4, 1805. [*Phil. Trans.* 1805, *p.* 316.]

In this paper the author relates circumstantially the series of operations by which he was led to the original discovery of palladium; and as he had an opportunity during the solution of a considerable quantity of platina, of making many observations that have not occurred to others, he undertakes, on the present occasion, to mention those which are most worthy of notice.

He remarks, that the gold which is usually found with platina is a constituent part of the ore of platina itself, when the grains are carefully selected.

The metals iridium and osmium, on the contrary, which were extracted by Mr. Tennant from the black powder that remains after solution of the ore of platina, Dr. Wollaston observes, are not only to be found in that powder which is extricated by solution from the interior of the grains of crude platina, but there exist also other grains originally distinct from those of platina, and consisting of these metals only.

These grains, which he considers as the proper ore of iridium mineralized by osmium, are harder than those of platina, are more brittle under the hammer, and when broken appear to be laminated.

The specific gravity of these grains, he says, is very remarkable, being greater than that of the ore of platina, which in his experiments has not exceeded 17·7, while that of the former is as much as 19·5. It would naturally be supposed that such a density might arise from the presence of a large quantity of platina in them; but the author did not succeed in obtaining any platina from these grains.

Among the various substances that may be separated from the ore of platina by washing, he notices also certain minute crystals of the colour of the ruby. Of these he gives a particular description, but does not undertake the analysis, on account of the very small quantity which he could obtain.

The author next proceeds to the solution of platina, from which he first precipitates the greater part of the platina pure, by sal ammoniac, and the remainder in an impure state by iron, a second metallic precipitate, which he observes consists of various metals intermixed.

It was in attempting to analyse this second metallic precipitate, that he first discovered palladium. After separating from it a quantity of lead, and some iron by muriatic acid, and dissolving out some copper by dilute nitrous acid, he was endeavouring to extract the remainder of the copper by a stronger nitrous acid, when he remarked that the colour of the solution, instead of being blue, as before, turned to a dark brown, in consequence of the solution of some other metallic ingredient. The first thought which occurred to him was, that some iron had remained, and had communicated this colour to the solution; but when he considered that this substance had been more slowly acted upon than copper, he relinquished that hypothesis, and, endeavouring to precipitate the metal by a clean plate of copper, he obtained a black powder, which was redissolved in nitrous acid, and formed a red solution.

The solubility of this precipitate in nitrous acid, showed that it did not consist either of gold or platina: the colour of the solution proved equally that it was neither silver nor mercury; and since the precipitation by copper excluded the supposition of all other known metals, he presumed that he was engaged with a new metallic body, but was not fully satisfied of its existence until he had afterwards precipitated it by mercury, with which it formed an amalgam. By treating this amalgam he procured, in a pure state, the metal to which he afterwards gave the name Palladium, from the planet which had been discovered, nearly at the same time, by Dr. Olbers.

There were various considerations arising out of the preceding experiments, which induced him to consider this as a new simple metal; but since it was possible he might be deceived, he undertook a course of experiments for the purpose of obviating all possible objections. He formed alloys with many different metals, dissolved it in various acids, and, having recovered it from the alloys and solutions so formed, he found it to remain unaltered, retaining its original properties, being nearly infusible by itself, but easily fused with sulphur, with arsenic, or with phosphorus; soluble in nitrous acid, and precipitated from thence by green sulphate of iron, by muriate of tin, by prussiates, and by hydro-sulphurets.

When he found all his endeavours to decompose this substance ineffectual, he became more confident of its being a new simple metal, and accordingly published a concise delineation of its character, but avoided directing the attention of chemists to the source from whence it had been obtained, and thereby reserved to himself a more deliberate examination of many phenomena that yet remained unexplained in the analysis of platina, by which he was subsequently led to the discovery of rhodium, another metallic substance, already published in the last volume of our Transactions.

Experiments on a Mineral Substance formerly supposed to be Zeolite; with some Remarks on two Species of Uran-glimmer. By the Rev. William Gregor. *Communicated by* Charles Hatchett, *Esq. F.R.S.* Read July 4, 1805. [*Phil. Trans.* 1805, *p.* 331.]

The mineral substance treated of in this paper, is similar to that of which Mr. Davy, some months ago, gave an account, under the title of Hydrargyllite or Wavellite. That which is now described by Mr. Gregor is produced from a mine called Stenra Gwyn, in the county of Cornwall.

Two species of this substance, the author says, are found in the above-mentioned mine; the first, and most common, consists of an assemblage of minute and delicate crystals, in radiated tufts, attached to quartz crystals. These crystals are in general white and transparent; sometimes, however, they have a yellowish hue. They vary considerably in their size, but seldom exceed a quarter of an inch in length.

Among these crystals are frequently seen two kinds of crystalline laminæ; one of them being in the form of parallelopipedons, with truncated angles, and of a green colour; the other forming an assemblage of square plates, varying in thickness, and the angles of which are not always coincident; these are of a bright wax yellow. This last kind is also found adhering to the sides of quartz crystals, in the cavities of granite.

The other species of the substance here treated of, consists of crystals closely compacted together in the form of mammillary protuberances, generally of the size of small peas, and forming a stratum about one eighth of an inch thick, upon quartz, in the cavities or fissures of compact granite. The striæ of these mammillæ diverge from a centre, like zeolite.

The detached crystals of the first species are easily reduced to powder. Their specific gravity, at 56° Fahr., was found to be 2·22. The second, or more compact species, is sufficiently hard to scratch calcareous spar: its specific gravity, at the temperature of 55°, was 2·253.

The crystals of the first species, when suddenly exposed to the action of the blowpipe, decrepitate; if gradually exposed to its action, they grow opake, but show no signs of fusion, even under the strongest heat. Both species, when exposed for some time to a red heat, experience a diminution in weight of about 30 per cent.

Some other experiments upon these substances are related, and a very minute account of the mode in which they were analysed is given; of this we must necessarily confine ourselves to give merely the results.

Fifty grains of the crystals of the first species yielded alumina $29\frac{1}{11}$ grs.; silica, $3\frac{7}{11}$ grs.; oxide of iron, $\frac{3}{11}$ grs.; lime, $\frac{1}{11}$ grs.; volatilized matter, $14\frac{1}{11}$ grs.

The sum total of these is	$47\frac{2}{11}$ grs.
Consequently the loss was	$2\frac{9}{11}$ grs.
	50

The silica and the lime, Mr. Gregor considers as essential to the composition of this mineral, as he has always discovered them, even in the purest specimens.

In order to examine the nature of the volatilized matter, the author submitted some of the crystals to distillation. A fluid passed over into the receiver, and a white crust was formed in the arch and neck of the retort. The fluid had an empyreumatic smell, very similar to that observed in the fluid distilled from the white crust that surrounds flint. It changed litmus paper to a faint reddish hue. A variety of experiments were made upon the white crust, from the results of which it appeared, that it consisted in part, at least, of an acid, which did not seem to be either the phosphoric or fluoric; nor did its properties entirely agree with those of the oxalic acid, although many of them were similar to those of that acid. A part of the forementioned crust, which firmly adhered to the neck of the retort, was found to contain a portion of lead; this, Mr. Gregor ascribes to the action of the acid on the retort.

Some of the Barnstaple mineral was also tried, and was found likewise to produce the above-mentioned white crust. Mr. Gregor now makes some remarks on the yellow and green crystals already mentioned as accompanying the mineral here treated of, which he says he at first considered as similar to the two species of Uran-glimmer examined by Klaproth. The specific gravity of the yellow crystals, at 45° Fahr., was 2·19. Exposed to the blowpipe, they decrepitated violently. They are taken up by phosphate of ammonia and soda without effervescence, and communicate a light emerald green colour to the fused globule. By exposure to a red heat they become of a brassy colour, and lose nearly a third part of their weight.

Several other experiments upon them are related, but their scarcity has, Mr. Gregor says, precluded him from operating on a quantity sufficient for a regular analysis. But he has detected in them oxide of lead, lime, and silica, which have not hitherto been considered as ingredients of Uran-glimmer.

The substance also, which in his experiments was held in solution by ammonia, had some peculiar properties which appeared to distinguish it from uranium.

The green crystals, the author says, do not differ from the yellow, except in containing a little of the oxide of copper.

The Croonian Lecture on the Arrangement and mechanical Action of the Muscles of Fishes. By Anthony Carlisle, *Esq. F.R.S. F.L.S.* Read November 7, 1805. [*Phil. Trans.* 1806, *p.* 1.]

The muscles of fishes, Mr. Carlisle says, are constructed very differently from those of the other natural classes of animals. The medium in which fishes reside, the form of their bodies, and the instruments employed for their progressive motion, give them a character peculiarly distinct from the rest of the animal creation. Their skeleton is simple, and their proportion of muscular flesh is remark-

ably large; but the muscles have no tendinous chords, their insertions being always fleshy. There are, however, semi-transparent pearly tendons placed between the plates of muscles, which give origin to a series of short muscular fibres, passing nearly at right angles between the surfaces of the adjoining plates.

The progressive motion of fishes, our author says, is chiefly performed by the flexions of the trunk and tail; the pairs of fins, which some have considered as analogous to feet, being only employed for the purposes of turning, stopping, altering the position of the fish towards the horizon, and keeping the back upwards. The single fins appear to prevent the rolling of the body whilst the tail is employed to impel it forwards. Each of the fins, which are in pairs, is capable of four motions, viz. of flexion and extension, like oars, and of expanding the rays, and closing them.

Mr. Carlisle now (taking the Cod as a standard of comparison,) describes particularly the mode in which the various motions here spoken of are performed, and then relates some experiments made to determine the effect of the fins on the motions of fishes. For this purpose a number of dace, equal in size, were put into a large vessel of water, and the pectoral fins of one of them being cut off, it was replaced with the others. The result was, that the progressive motion of the fish was not at all impeded; but its head inclined downwards; and when it attempted to ascend, the effort was attended with difficulty.

From another fish, both the pectoral and abdominal fins were taken. The fish remained at the bottom of the vessel, and could not be made to ascend. Its progressive motion was not perceptibly more slow; but when the tail acted, the body showed a tendency to roll, and the single fins were widely expanded, as if to counteract this effect.

From a third fish the single fins were removed. This produced an evident tendency to turn round, and the pectoral fins were kept constantly extended, to obviate that motion.

From a fourth fish all the fins were removed. Its back was kept in a vertical position, whilst at rest, by the expansion of the tail; but it rolled half round at every attempt to move.

From a fifth fish the tail was cut off as close to the body as possible. The progressive motion of the fish was considerably impeded, and the flexions of the spine were much increased; but neither the pectoral nor the abdominal fins seemed to be more actively employed.

From a sixth fish all the fins and the tail were removed. It remained without motion, floating near the surface of the water, with its belly upward.

The above experiments were repeated on the roach, the gudgeon, and the minnow, with similar results.

Mr. Carlisle now observes, that the muscles of fishes differ very materially in their structure from those of other animals; that they are apparently more homogeneous; that their fibres are not so much fasciculated, but run more parallel to each other, and are always

comparatively shorter; and that they become corrugated at the temperature of 156° of Fahrenheit, when their tendinous and ligamentous attachments are dissolved, and their serous juices coagulated. He then proceeds to give a very minute description of the situation and arrangement of the various series which form what are called the lateral muscles of the body. The nerves belonging to these muscles are also described; and mention is made of loose transparent vesicles about the size of a millet-seed, containing a white substance like carbonate of lime, which vesicles are found within the sheath of the nerves, at the point of their junctions.

The rate at which fishes move through so dense a medium as water, is, our author says, very remarkable; and although this may be partly accounted for by the large proportion of muscles, and their advantageous application, yet the power would be inadequate to the effect if it were not suddenly exerted: this appears from the slow progress of eels, and such other fishes as, from their length and flexibility, are incapable of giving a sudden lateral stroke.

But the quickness and force of action in the muscles of fishes is counterpoised by the short duration of their power. Those accustomed to the diversion of angling, know how soon the strength of fishes is exhausted; for if the hooked fish is kept in constant action, it soon loses the ability to preserve its balance, and turns upon its side. This, Mr. Carlisle says, has been vulgarly attributed to drowning, in consequence of the mouth being closed upon the hook; but the same effects, he says, take place when the hook is fastened to the side or the tail; and he thinks that this prostration of strength may depend partly on fear, and partly on interrupted respiration; since fishes, when swimming rapidly, keep the *membranæ branchiosteaæ* closed, and when nearly exhausted, act violently with their gills.

The shortness of the muscular fibres, and the multiplied ramifications of the blood-vessels, are probably peculiar adaptations for the purpose of gaining velocity of action, which seems invariably connected with a very limited duration of it. Such examples, our author thinks, form an obvious contrast with the muscular structure of slow-moving animals, and with those partial arrangements where unusual continuance of action is concomitant.

Since Mr. Carlisle's former communications respecting the arteries of slow-moving muscles, another instance has been pointed out to him by Mr. Macartney, in the muscles of the feet and toes of birds, which seems to be an adaptation for the alternate rest of their limbs while sleeping.

The muscles of the human body which perform the most rapid actions, have their fibres subdivided by transverse tendons, or are arranged in a penniform direction. The *semi-tendinosus* and *semi-membranosus* of the thigh are thus constructed, and the *recti abdominis* are divided into short masses by transverse tendons. All these muscles cooperate in the action of leaping.

These observations, the author thinks, tend to explain that diversity

which is found in the lengths of various muscles that act together; as by that means organs of velocity are joined with those of power.

The Bakerian *Lecture on the Force of Percussion. By* William Hyde Wollaston, *M.D. Sec. R.S.* Read November 14, 1805. [*Phil. Trans.* 1806, *p.* 13.]

The force of percussion is a subject, respecting the estimation of which a controversy has subsisted for more than a century past between different classes of philosophers. For although it is agreed that when unequal bodies move with the same velocity, the forces are as their quantities of matter; yet when equal bodies move with unequal velocities, there are two methods of estimating the comparative forces of such bodies. Leibnitz and his followers conceive the forces to vary as the squares of the velocities; while their opponents maintain that the forces are in the simple ratio of the velocities of the bodies respectively. The latter have been considered as Newtonians; but Dr. Wollaston endeavours to show that they can derive no support from any expressions of Newton.

In order to explain the grounds for each opinion, the author proposes the following experiment.

He supposes a ball of clay to be suspended at rest, having two similar and equal pegs slightly inserted into its opposite sides; and he supposes two other bodies, A and B, which are to each other in the proportion of 2 to 1, to strike at the same instant against the opposite pegs, with velocities which are in the proportion of 1 to 2. In this case, the ball of clay would not be moved from its place to either side; nevertheless, the peg impelled by the smaller body B, which has the double velocity, would be found to have penetrated twice as far into the clay as the peg impelled by the larger body A.

It is, Dr. Wollaston says, unnecessary to make the above experiment precisely as it is here stated, because the results are admitted as facts by both parties; but upon these facts they reason differently. One party, observing that the ball of clay remains unmoved, considers the proof indisputable, that the action of the body A is equal to that of the body B, as they would be led to expect, because their *momenta* are equal. Their opponents think it equally proved, by the unequal depths to which the pegs have penetrated, that the causes of these effects are unequal, as they would have expected, from considering the forces as proportional to the squares of the velocities.

The former party observe, in this experiment, that equal *momenta* can resist equal pressures during the same *time*; the other party attend to the *spaces* through which the same moving force is exerted, and finding them to be in the proportion of 2 to 1, observe that the *vis viva* of a body in motion is justly estimated by the magnitude and the square of the velocity jointly,—a multiple to which Dr. Wollaston has thought it convenient to give the name of Impetus.

This latter conception, of a quantity of force as a *vis motrix* extended through space, rather than continued for a certain time, is an

idea which, the author observes, arises naturally from the daily occupations of men, since any quantity of work performed is always estimated by the extent of effect resulting from their exertions. Thus it is well known that the raising of any great weight 40 feet would require four times as much labour as would be requisite to raise an equal weight 10 feet. And if weights so raised were suffered to fall freely, the squares of the velocities acquired would be in proportion to the quantity of labour, that is, as 4 to 1; and if their forces were employed in driving piles, the effects produced would be in that same ratio.

This species of force has, by Smeaton, been aptly denominated mechanic force; and when by force of percussion is meant the quantity of mechanic force which a body in motion can exert, the author apprehends it cannot be controverted that the said force is in proportion to the magnitude of the body, and the square of its velocity jointly.

But of this force Newton nowhere treats, and consequently gives no definition of it; on the contrary, in the preface to the Principia, he expressly says, that he writes "de potentiis non manualibus, sed naturalibus;" and again, in the Scholium to the laws of motion, he says, "Cæterum mechanicam tractare, non est hujus instituti."

It is also evident, that in the third law of motion, when Newton asserts that action is equal to reaction, he means only that the moving forces, or pressures opposed to each other, are necessarily equal. Other persons, however, have interpreted the third law differently, and conceive also a species of accumulated force, which is capable of resisting a given pressure, during a time that is proportional to the *momentum*, or *quantitas motûs*.

If it be of any real utility to give the name of force to such a complex idea of *vis motrix* continued for any certain time, the author recommends that it should be always distinguished by some such appellation as *momentous* force, as he apprehends that, for want of this distinction, both writers and readers of disquisitions upon this subject have confounded and compared together *vis motrix*, *momentum*, and *vis mechanica*; quantities that are all of them totally dissimilar, and bear no more comparison to each other than lines to surfaces, or surfaces to solids.

In practical mechanics, however, it is at least very rarely that that *momentum* of bodies is an object of consideration; since the extent and value of any effect to be produced depends upon the *quantitas mechanica* of the force applied, or in other words, the space through which any moving force is exerted.

Dr. Wollaston, in the next place, compares the forces of the different bodies by means which he is inclined to think have not been taken notice of by any writer on this question; and he shows, that when the whole energy of a body A is employed without loss, in giving velocity to a second body B, the impetus which B receives is, in all cases, equal to that of A, the squares of their velocities being in the reciprocal ratio of the bodies.

As a simple case of entire transfer of force from A to B, it is evident that if A were allowed to ascend to the height due to its velocity, and if by any mechanical contrivance, of lever or otherwise, the body B were to be raised by the descent of A, their heights of ascent would be reciprocally as the bodies; consequently, that the *square* of the velocity to be acquired by the free descent of B, would be, to that of A, in the above-mentioned ratio, and the quantity of mechanic force so estimated would be preserved unaltered.

But, on the contrary, the *momentum*, which is in the simple reciprocal ratio of the bodies, would be increased by such means in the subduplicate ratio of the bodies that might be employed; and if *momentum* were really a force efficient in proportion to its estimated magnitude, it should not only be capable of reproducing the original quantity, but the additional force, thus acquired, might be employed for counteracting the usual resistances, and perpetual motion would be easily produced. But since the impetus, or mechanic force, remains unaltered, it is evident that the utmost that B could effect, in return, would be the reproduction of A's velocity, and restitution of its former force, neither increased nor diminished, excepting by the necessary imperfection of machinery.

The possibility of perpetual motion is consequently inconsistent with those principles which measure the quantity of force by the quantity of its extended effects, or by the square of the velocity which it can produce.

Since we can, at pleasure, by means of any mechanic force, consisting of a *vis motrix* extended through a given space, give motion to a body for the purpose of employing its impetus in the production of any sudden effect, or can, on the contrary, occasion a moving body to ascend, and thus resolve its impetus into a moving force ready to exert itself through a determinate space of descent, capable of producing precisely the same quantity of mechanic effect; the force depending on impetus may justly be said to be a force of the same kind as any other mechanic force, and may be strictly compared with them as to quantity.

In this manner, the author says, we may even compare the force of a body in motion, with the same kind of force contained in a given quantity of gunpowder, and may say that we have the same quantity of mechanic force at command, whether we have one pound of gunpowder, or the weight which it would raise to the height of 30 feet, actually raised to that height, and ready to be let down gradually; or the same weight possessing its original velocity of ascent, to be employed in any sudden exertion.

By employing the same measure, we have a distinct expression for the quantity of mechanic force given to a steam-engine by a peck or by a bushel of coals; and are enabled to compare its effect with the quantity of work which one or more horses may have performed in a day. In short, whether we are considering the sources of extended exertion, or of accumulated energy,—whether we compare the accumulated forces themselves by their gradual or their sudden effects,

the idea of mechanic force, in practice, is always the same, and is proportional to the *space* through which any *moving force* is exerted, or to the *square* of the velocity of a body in which such force is accumulated.

Mémoire sur les Quantités imaginaires. Par M. Buée. *Communicated by* William Morgan, *Esq. F.R.S.* Read June 20, 1805. [*Phil. Trans.* 1806, *p.* 23.]

Chemical Experiments on Guaiacum. By Mr. William Brande. *Communicated by* Charles Hatchett, *Esq. F.R.S.* Read December 19, 1805. [*Phil. Trans.* 1806, *p.* 89.]

No one of the resins, Mr. Brande observes, possesses so many curious properties as that called Guaiacum; and he thinks it remarkable, that although many of the alterations it undergoes, when heated with different solvents, have been mentioned by various authors, it has not excited a more particular attention.

After noticing its more obvious properties, of which we shall only repeat, that when pulverized, it is of a gray colour, but gradually becomes greenish by exposure to the air, he proceeds to examine the action of various solvents upon it.

The first solvent tried by Mr. Brande was water; about 9 per cent. of extractive matter was taken up, and the solution appeared also to contain a small portion of lime. Alcohol, which was next tried, dissolved nearly the whole of the guaiacum, leaving only about 5 per cent. of extraneous matter. The effects of water, of various acids, and of alkalies, upon this solution, are then noticed. Water forms a milky fluid, which passes the filter. Muriatic acid throws down an ash-coloured precipitate. Liquid oxymuriatic acid forms a precipitate of a pale blue colour. Sulphuric acid forms one of a pale green. Acetic acid does not form any precipitate; nor does nitric acid until after the expiration of some hours, unless water be added, in which case a precipitate may be sooner obtained. This precipitate is of a green or a blue colour; whereas that which forms spontaneously is brown. Alkalies do not form any precipitate when added to the solution of guaiacum in alcohol.

Guaiacum is less soluble in sulphuric ether than in alcohol, but the properties of the two solutions are nearly similar.

Muriatic acid dissolves only a small portion of guaiacum. Sulphuric acid forms with that substance a deep red liquid, which, when fresh prepared, deposits a lilac-coloured precipitate on the addition of water. The effects of nitric acid on guaiacum are minutely examined, of which we shall only mention, that this acid, when its specific gravity was 1·39, completely dissolved guaiacum, which solution, after standing some hours, deposited a quantity of crystallized oxalic acid; but when the nitric acid was diluted, a slight effervescence took place, and a part only of the resin was dissolved, the remainder being converted into a brown substance, which was similar

to the brown precipitate obtained, by nitric acid, from the solution of guaiacum in alcohol, and possessed the properties of a resin in greater perfection than guaiacum itself. If successive portions of nitric acid be added to the above-mentioned residuum, or if a large quantity of that acid is employed so as to form a complete solution, a product may be obtained, by evaporation, which is equally soluble in water and in alcohol; both which solutions have an astringent bitter taste.

Guaiacum is soluble in the pure and in the carbonated alkalies. The precipitates formed from these solutions, by dilute sulphuric acid and by muriatic acid, were of a flesh colour, and approached to the nature of extract; being less acted upon by sulphuric ether, but more soluble in boiling water than guaiacum.

Mr. Brande now proceeds to the analysis, by distillation, of the substance here treated of. By this method he obtained, from 100 grains, the following products:—

Acidulated water	5·5
Thick brown oil, becoming turbid on cooling	24·5
Thin empyreumatic oil	30·0
Coal remaining in the retort	30·5
Mixed gases, chiefly carbonic acid and carbonated hydrogen	9·0
	99·5

The coal, on incineration, yielded four grains of lime, but no alkali could be discovered.

From the foregoing experiments it appears, that although guaiacum possesses many of the properties common to resins, it differs from them in the following circumstances.

1. By affording a portion of vegetable extract.

2. By the alterations which take place in it when submitted to the action of bodies which readily communicate oxygen, such as nitric and oxymuriatic acids, and by the rapidity with which it is dissolved in the former.

3. By being capable of being converted into a more perfect resin, in which it resembles the green resin that constitutes the colouring matter of leaves.

4. By yielding oxalic acid.

5. By the quantity of charcoal and lime obtained from it by distillation.

These circumstances, the author says, shows that guaiacum differs not only from the substances denominated resins, but also that it differs from those which are called balsams, gum-resins, gums, and extracts; and he thinks we may, for the present, consider guaiacum as composed of a resin, modified by the vegetable extractive principle, so that it may perhaps, without impropriety, be defined by the term Extracto-resin.

In a postscript Mr. Brande observes, that the action of oxygen on some other resinous bodies is very remarkable. By digesting mastic

in alcohol, a partial solution is formed, leaving an elastic substance, which is said to possess the properties of caoutchouc, but which becomes hard by exposure to the air.

The author has remarked, that the portion of mastic dissolved in the alcohol may be precipitated from it by water, and that this precipitate possesses the properties of a pure resin; but when a stream of oxymuriatic acid gas was passed through the solution, a tough elastic substance was thrown down, which became brittle when dry: this precipitate was soluble in boiling alcohol, but separated from it as the solution became cool. Its properties, therefore, approached in some measure to those of the original insoluble part.

On the Direction of the Radicle and Germen during the Vegetation of Seeds. By Thomas Andrew Knight, *Esq. F.R.S. In a Letter to the Right Hon. Sir* Joseph Banks, *K.B. P.R.S.* Read January 9, 1806. [*Phil. Trans.* 1806, *p.* 99.]

It is, Mr. Knight observes, very well known, that in whatever position a seed is placed to germinate, its radicle always makes an effort to descend towards the centre of the earth, whilst the elongated germen takes a precisely opposite direction: and it has been proved by Du Hamel, that if a seed, during its germination, be frequently inverted, the points, both of the radicle and germen, will return to their first direction. These opposite effects have, by some naturalists, been attributed to gravitation; and Mr. Knight conceived, that if they really proceeded from that cause, those effects would take place only whilst the seed remained at rest, in the same position with respect to the attraction of the earth, and that the operation of gravitation would be suspended by a constant and rapid change of position in the germinating seed, and might be counteracted by the agency of centrifugal force. In order to determine how far the above opinion was well founded, he made the following experiments:—

Having a strong rill of water passing through his garden, he contrived, by its means, to give motion, vertically, to a wheel of eleven inches diameter. Round the circumference of this wheel, several seeds of the garden-bean, which had been previously soaked in water, were bound in such a manner that their radicles were made to point in every direction. The wheel made rather more than 150 revolutions in a minute.

In a few days the seeds began to germinate, and Mr. Knight had the pleasure to see that the radicles, in whatever direction they were protruded, turned their points outwards from the circumference of the wheel, and in their subsequent growth receded still further from it. The germens, on the contrary, took the opposite direction; and in a few days their points met at the centre of the wheel. Three of these plants were suffered to remain on the wheel; their stems soon extended beyond its centre, but their points returned, and met again at the centre.

As Mr. Knight conceived that some slight objections might be urged against the conclusions he was inclined to draw from the above experiment, he repeated it in a different manner, by adding to his former apparatus another wheel, also of eleven inches diameter, which moved horizontally, and to which he could give different degrees of velocity. Round the circumference of this horizontal wheel, seeds of the garden-bean were bound, as in the former experiment, and the wheel was made to perform 250 revolutions in a minute. The effect produced by this motion soon became obvious; for the radicles now pointed downwards about ten degrees below the horizontal line of the wheel's motion, whilst the germens pointed the same number of degrees above it: but when the motion of the wheel was diminished to 80 revolutions in a minute, the radicles pointed about 45 degrees below the horizontal line, and the germen as much above it; the one always receding from the axis of the wheel, the other approaching to it.

The foregoing experiments, the author thinks, prove that the radicles of the germinating seeds are made to descend, and the germens to ascend, by some external cause, and not by any power inherent in vegetable life; and he sees little reason to doubt that gravitation is the principal if not the only agent employed in this case by nature. The radicle, he says, is increased in length only by parts successively added to its point; whereas the germen, on the contrary, is elongated by a general extension of its parts previously organized; and its vessels and fibres appear to extend themselves in proportion to the quantity of nutriment they receive. When the germen deviates from a perpendicular direction, the sap accumulates on its under side; and consequently, as the vessels and fibres on that side elongate more rapidly than those of the upper side, the point of the germen must always turn upwards. This increased elongation of the vessels and fibres of the under side produces also the most extensive effects in the subsequent growth of the trunks and branches of trees. The immediate effect of gravitation, Mr. Knight says, is to occasion the depression of the branches; but, by the above-mentioned increased longitudinal extension of the under side, their depression is prevented, and they are even enabled to raise themselves above their natural level.

It has, however, been objected by Du Hamel, that gravitation can have little influence on the germen when it points perpendicularly downwards. To obviate this objection, Mr. Knight made many experiments on the seeds of the horse-chestnut and of the bean. The result was, that the radicle of the bean, when made to point perpendicularly upwards, formed a considerable curvature in the course of a few hours. The germen was more sluggish; but, in spite of any efforts made by the author to prevent it, constantly changed its direction in less than twenty-four hours.

It may also, Mr. Knight says, be objected, that few of the branches of trees rise perpendicularly upwards, and that their roots always spread horizontally. Respecting the first of these objections, he ob-

serves, that luxuriant shoots, which abound in sap, constantly turn upwards, and endeavour to acquire a perpendicular direction; but that the feeble and more slender shoots grow in almost every direction, probably from their fibres being more dry, and their vessels less amply supplied with sap, so that they are less affected by gravitation. To the second objection, Mr. Knight answers, that the compression of the radicle, as it penetrates the soil, obstructs the motion of the sap, and occasions the generation of numerous lateral roots; and as their substance is less succulent than that of the radicle first emitted, they are less obedient to gravitation, and consequently extend horizontally in every direction. Respecting the tap-root of the oak, the author says he has examined at least 20,000 trees of that species, and never found one tree that possessed a tap-root; he therefore thinks he may be allowed to doubt the existence of such a root.

A third Series of Experiments on an artificial Substance, which possesses the principal characteristic Properties of Tannin; with some Remarks on Coal. By Charles Hatchett, *Esq. F.R.S.* Read January 16, 1806. [*Phil. Trans.* 1806, *p.* 109.]

Mr. Hatchett, in his former communications on this subject, gave some account of the effects produced by sulphuric acid upon turpentine, resin, and camphor. He now states the results of a variety of experiments made with that acid upon a great number of resins, balsams, gum-resins, and gums; from which it appears, that sulphuric acid almost immediately dissolved the resins, forming transparent brown solutions, which gradually became black; that the solutions of the balsams and of guaiacum were at first of a deep crimson colour, slightly inclining to brown; and that caoutchouc and elastic bitumen were not dissolved, but, after a long digestion, were only superficially carbonized.

Turpentine, common resin, elemi, tucamahaca, mastic, copaiba, copal, camphor, benzoin, the balsams of Tolu and of Peru, assafœtida, and amber, yielded a large proportion of the tanning substance; so also did oil of turpentine.

Asphaltum yielded only a small portion of that substance; and some slight traces of it were obtained from gum arabic and from gum tragacanth; but none was produced from guaiacum, dragon's blood, myrrh, gum ammoniac, olibanum, gamboge, caoutchouc, elastic bitumen, liquorice, and manna. Mr. Hatchett thinks, however, that some of these would have yielded it, had not the digestion with nitric acid been too long continued.

Olive oil was partly converted into the tanning substance; so also were linseed oil, wax, and animal fat. In the experiment with linseed oil, a portion was left undissolved: this portion appeared to retain many of the properties of an inspissated fat oil. In the experiment made with wax, a white substance was obtained, which was found to possess the properties of spermaceti. In that with animal fat (in which the kidney-fat of veal was employed), a great

portion of a grayish-black substance was produced, which was highly inflammable, was easily melted, and was readily dissolved in cold alcohol; from which, like the resins, it might be precipitated by water.

From coagulated albumen, and from prepared muscular fibre, nothing but coal could be obtained.

In the above experiments there appeared to be a certain period of the process when the production of the tanning substance arrived at its maximum; after which a gradual diminution, and at length a total destruction of it, took place, and it became mere coal.

Some experiments are now related, made with nitric acid, on the elastic bitumen, and on several kinds of coal. The result was, that from elastic bitumen, common pit-coal, Cannel-coal, and asphaltum, there was obtained, not only the tanning substance, but also another substance, which possessed properties intermediate between those of resin and those of vegetable extractive matter; but this substance might, by digestion in nitric acid, be converted into the tanning substance. From Kilkenny-coal, and from two other kinds of coal, one from Wales, the other from North America, none of the above-mentioned resinous substances were obtained.

Mr. Hatchett now proceeds to mention a variety of experiments made on horse-chestnuts, and on their peels. From these it appeared, that the small portion of tannin originally contained in horse-chestnut peels is destroyed by the process of roasting; but that the brown decoctions of the roasted horse-chestnuts, and of their peels, might be made to afford the tannin matter, by the addition of nitric acid. The above brown decoctions appeared to contain carbon, combined with oxygen, sufficient to give it many of the properties of coal; but the compound is nevertheless capable of being dissolved by water with great facility.

Solutions similar to the above may, our author thinks, be obtained whenever vegetable matter undergoes the putrefactive process, as in dunghills, &c. He examined the brown liquor that runs from walnut-peels when kept in a heap for a certain time, and found that, like the decoctions above mentioned, it contained carbon in a state approaching to coal, and that, by the addition of nitric acid, a small portion of the tanning substance might be procured from it.

Some experiments were likewise made upon galls; the results of which showed, that the natural tannin contained in them is destroyed by nitric acid; that it is also diminished, and ultimately destroyed, by roasting; but when the galls have not been so much roasted as to destroy the whole of the tannin, the remainder of that substance is destroyed by the addition of nitric acid, whilst, at the same time, a small portion of the artificial tannin is produced.

Results nearly similar were obtained from experiments upon oak-bark; and it also appeared, that when that bark was exhausted of its natural tannin, it might, by roasting and being treated with nitric acid, be made to yield the artificial tanning substance. This process was several times repeated upon the same portion of bark; and as it

still continued to yield the tanning substance, our author thinks it probable the process might be repeated until the whole of the bark became converted into the above substance.

From the foregoing experiments, and many others made by him, Mr. Hatchett thinks that the method of treating roasted vegetable substances here described is the most speedy and economical for obtaining the artificial tanning matter; and, as all refuse vegetables may be thus converted into that matter by simple and unexpensive means, he hopes the discovery may eventually be productive of some real public advantage.

In a former paper Mr. Hatchett observed, that he suspected the tannin found in some peat-moors was produced during the imperfect carbonization of the original vegetable substances: whether that is really the case, or whether it has been afforded by heath or other vegetables growing upon and near the peat, is, he says, still uncertain, as he has never been able to detect any tanning substance in peat, although he has examined a considerable number of varieties of it. The great facility with which tannin is dissolved by water causes it to be speedily extracted and drained from the substances which at first contained it: and that this facility of extraction extends to the most solid vegetable bodies, is shown by an experiment made by our author on a piece of oak from the submerged forest at Sutton, on the coast of Lincolnshire, described in the Phil. Trans. for the year 1799. This oak, by decoction, afforded extractive matter, but no traces of tannin could be perceived; yet, by incineration, it even afforded potash.

Peat, however, although it does not contain tannin, is, by the imperfect carbonization it has undergone, rendered capable of being converted, by treatment with nitric acid, into the artificial tanning substance, in the manner already mentioned with respect to roasted ligneous bodies.

In the following section of his paper, Mr. Hatchett compares the effect of the acetic, sulphuric, and nitric acids, upon resinous substances. The first of these he considers as the solvent of such substances, as it dissolves them speedily, without producing any apparent subsequent change in their natural properties; so that, by proper precipitants, they may be separated from that acid in an unaltered state. Sulphuric acid immediately dissolves resinous substances; but the moment the solution is complete, progressive alterations appear to take place in the dissolved substance, coal being the ultimate product.

The effects of nitric acid seem to be the reverse of those of the sulphuric; for by nitric acid the resins are converted into a brittle porous substance, then into a soluble product intermediate between extractive matter and resin, which product is converted into the first variety of the tanning substance; beyond which our author has not been able to effect any change. A table of the quantity of coal remaining after the treatment of various resinous substances with sulphuric acid is now given; respecting which we shall

only remark, that a much greater proportion of coal is obtained from those substances by means of the above acid than can be obtained by distillation.

Two experiments on the humid formation of coal are also described: from one of these it appears that oak-wood may, by sulphuric acid, be converted into a coal which is very different from charcoal, and which, by its mode of burning, and by its not affording any alkali, resembles those mineral coals that do not contain bitumen.

The other experiment shows that oak-wood may also be converted into a sort of coal by muriatic acid; but this coal retains some vegetable characters, although no alkali can be obtained from its ashes.

Mr. Hatchett now proceeds to make some remarks on the natural formation of coal. After stating the various theories that have been formed on that subject, he considers as the most probable the theory which ascribes the principal origin of coal to vegetable substances; that idea of its origin being, he says, corroborated by the greater number of geological facts. The observations, however, that have been made upon the submerged wood found at Sutton and other places, show, our author thinks, that vegetable substances, buried under the sea or under the earth, are not, merely by such means, converted even into the most imperfect sort of coal; some other process being evidently necessary to produce this change, which in a former paper he endeavoured to demonstrate to be progressive.

That some sorts of coal are of vegetable origin, there cannot, Mr. Hatchett says, be any doubt: several of them, as the Bovey coal, the Sussex coal, the surturbrand, &c. not only still retain some of their external vegetable characters, but also yield resin,—a substance allowed to belong exclusively to organized natural bodies. Some mineralogists, however, have attempted to distinguish the above-mentioned coals from others, which they denominate True Mineral Coals: but it has in the former part of this paper been shown, that when pit-coal, Cannel-coal, and asphaltum, (which are considered as of mineral origin,) are subjected to the action of nitric acid, and the process is stopped at a proper period, there remains a substance which is intermediate between resin and vegetable extractive matter. It has also been stated, that, by similar means, a substance possessing nearly the same properties may be obtained from the known vegetable resins.

Our author indeed admits that bitumen has never been formed by any artificial process, and that he has himself attempted it, in various ways, without success: yet we may conclude, from what has been already said, that bitumen is a modification of the resinous and oily parts of vegetables, produced by some process of nature, operated by gradual means on immense masses; and we have, he thinks, great reason to conclude that the agent employed by nature in the formation of coal and bitumen is either the muriatic or the sulphuric acid. Common salt, however, is never found in coal-mines, except when they are in the vicinity of salt-springs; whilst, on the contrary, py-

rites, sulphate of iron, and alum, are commonly found in such mines; from which circumstances, together with the sulphureous odour emitted by most of the mineral coals when burned, the agency of sulphuric acid is strongly evinced; and, as we have already observed, the coals formed artificially from vegetable substances, by means of sulphuric acid, bear a strong resemblance to the mineral coals, not only in their external characters, but also in their other properties.

Mr. Hatchett intends, he says, to relinquish any further prosecution of this subject for the present; but he entertains such sanguine expectations of its proving economically useful, that he strongly recommends the prosecution of the inquiry, particularly of that part which relates to roasted vegetable substances and to peat.

The Application of a Method of Differences to the Species of Series whose Sums are obtained by Mr. Landen, *by the Help of impossible Quantities. By Mr.* Benjamin Gompertz. *Communicated by the Rev.* Nevil Maskelyne, *D.D. Astronomer Royal, F.R.S.* Read February 13, 1806. [*Phil. Trans.* 1806, *p.* 147.]

The nature of this paper is such, as renders it absolutely incapable of abridgement. By way of introduction to it the author observes, that having some years back, when reading the learned Mr. Landen's fifth memoir, discovered the manner of applying a method of differences to the species of series whose sums are there obtained by the help of impossible quantities, and having since extended that application, he now ventures to offer it to the consideration of others.

The practice of this method, in most cases, appears, he says, extremely simple, and on that account he is almost induced to imagine that it has already been considered by mathematicians. And he acknowledges that, since the greatest part of the paper was written, he has, in Euler's Institutiones Calculi Integralis, met with two simple series, which are in that work summed by multiplications similar to those employed in the investigation of the principal theorems contained in this paper. But whether that learned mathematician has pursued the method any further, he has not been able to ascertain.

Mr. Gompertz has purposely considered some of the series summed by Mr. Landen, in order to procure an opportunity of comparing both the results and methods; and as the series may have particular cases, in which both Mr. Landen's means and those of our author fail, he has added, towards the end, a general Scholium concerning the causes, circumstances, and consequences of such failure.

An Account of a small Lobe of the human prostate Gland, which has not before been taken notice of by Anatomists. By Everard Home, *Esq. F.R.S.* Read February 20, 1806. [*Phil. Trans.* 1806, *p.* 195.]

The subject of this paper is a portion of a gland which, from the smallness of its size, and the obscurity of its situation, has hitherto

escaped observation; and were it not for the change produced in it by disease, which sometimes enlarges it so much that it shuts up the urinary canal, it would, Mr. Home says, be little deserving of attention.

It is well known that the prostate gland is, in the latter periods of life, liable to enlarge; in that case there is frequently a nipple-like projection, which forms tumours, of different sizes, in the cavity of the bladder. These tumours have engaged the attention of surgeons from the time of Morgagni to the present day; but the peculiarities in the natural conformation of the gland which dispose it to form these tumours, have never been examined.

After stating the observations of Morgagni and of the late Mr. John Hunter upon the subject, Mr. Home says that his attention was directed to it by the examination of the prostate gland of an elderly person, who had died in consequence of this part having been diseased. The nipple-like process was very prominent, and a bridle nearly a quarter of an inch in breadth extended from the middle line of the tumour to the bulb of the urethra, where it insensibly disappeared. The usual rounded projection of the *caput gallinaginis* was not visible; and the space between the tumour in the bladder and the bulb of the urethra was unusually short; so that the bridle, which had evidently been formed by the membrane of the bladder adhering to that part of the prostate gland of which the tumour was composed, appeared to have drawn the bulb towards the tumour, and to have shortened the membranous part of the canal.

The above appearance of a bridle is more or less met with in all cases in which the nipple-like process occurs; but in so small a degree, that Mr. Home had not before been led to pay attention to it. He now thought it right to examine the prostate gland in its natural state, in order to ascertain whether any part of it is sufficiently detached to move independently of the rest of the gland; and as his professional avocations did not allow him sufficient time to make the requisite dissections, he committed that task to Mr. Brodie, demonstrator of anatomy to Mr. Wilson of Windmill-street. In consequence of Mr. Brodie's accurate examination of the part, a small rounded substance was discovered in the space between the two posterior portions of the gland: this substance was so much detached, that it seemed a distinct gland; and it so nearly resembled Cowper's gland in size and shape, that it appeared to be a gland of the same kind. It could not, however, be satisfactorily detached from the prostate gland, nor could any distinct duct be found leading from it into the bladder.

A similar examination of the part was made in five different subjects. The appearance was not exactly the same in any two of them; but our limits will not allow us to describe particularly the differences that were observed; we shall therefore only say, that the most distinct appearance of the part was found in a healthy subject of twenty-five years of age. In this subject the prominent body was imbedded, not only between the *vasa deferentia* and the bladder, but

also, in some measure, between the lateral portions of the prostate gland and the bladder. It was evidently a lobe of the prostate gland; and its ducts passed directly through the coats of the bladder, and opened immediately behind the verumontanum.

A still more distinct appearance of this lobe was afterwards found in a subject twenty-four years of age; a representation of which accompanies this paper.

This newly acquired anatomical fact enables us, Mr. Home says, to understand the nature of a disease, of which we could not have a clear idea while we were ignorant of the existence of the part in which it originates: it also enables us to explain various circumstances respecting the disease, particularly what to our author has ever appeared the greatest difficulty, namely, the protrusion of the tumour into the cavity of the bladder. This protrusion arises from the hard substance of the coats of the *vasa deferentia* being in close contact, and bound down upon the lobe; so that, from its first enlargement, it must press up the thin coats of the bladder. The situation of this lobe, and its connexion with the *vasa deferentia*, also render it liable to many causes of swelling, from which the body of the gland is free; since every irritation of the seminal vessels, or of their orifices, may be communicated to it by continuity of parts.

There is much reason, our author says, to believe that the diseased state of the lateral parts of the gland, so common in the later periods of life, has its origin in the lobe here described; for, in most of the cases examined by him, this lobe has been enlarged in a much greater degree, in proportion to its size, than any other part of the gland; and the difficulty in passing the urine, which comes on very early in the disease, is, Mr. Home thinks, owing to the enlargement of this lobe; since an enlargement of the lateral portions of the gland widens the canal instead of diminishing it. The enlargement of the lobe also occasions the bladder to retain a considerable part of the urine; and as the urine passes in a stream, and the quantity voided is sufficient, no suspicion is entertained of the cause of the frequency and distress in passing it; but they are referred to an irritable state of the coats of the bladder.

From the above observations it appears that the small lobe of the prostate gland here treated of is, from its situation and the circumstances in which it is placed, more liable to become diseased than any other part of the gland; and that it produces symptoms of danger and distress which are peculiar to itself, but which have been hitherto supposed to arise from the body of the gland becoming enlarged.

On the Quantity and Velocity of the Solar Motion. By William Herschel, *LL.D. F.R.S.* Read February 27, 1806. [*Phil. Trans.* 1806, *p.* 205.]

The present paper is a continuation of that communicated to the Society by Dr. Herschel last year, in which he considered the direc-

tion of the solar motion: he now proceeds to consider the quantity and velocity of that motion; and as, in the former paper, the proper motions, when reduced to one direction, were called quantities, to distinguish them from the velocities required in the moving stars to produce those motions, so also in the present inquiry, it will, the author says, be necessary to keep up the same distinction with respect to the velocity of the solar motion. To determine this, we ought to have in view the real motion of every star whose apparent motion we know; but as trials with a number of stars would be very inconvenient, Dr. Herschel makes use only of the six stars mentioned in his former paper, in laying down the method followed with all the rest.

He first considers the proportional distance of the stars; for till this is fixed upon, neither the parallactic nor the real motion of a star can, he says, be ascertained. And as it is not sufficiently satisfactory to divide the stars into a few magnitudes, and suppose these to represent their relative distances, he expresses the relative brightness of the six stars already alluded to, according to the notation formerly used by him for that purpose; and from that introduces, by using fractional distinctions, a more minute subdivision than has been commonly admitted. He then proposes the following arrangement, as expressing their proportional distance:—Sirius 1·00, Arcturus 1·20, Capella 1·25, Lyra 1·30, Aldebaran 1·40, Procyon 1·40.

Our author next considers the effect of the increase and decrease of the solar motion, and the conditions to be observed in the investigation of its quantity. He gives a table, calculated with a view to show that an increase or decrease of the solar motion will have a contrary effect upon the required real motion of different stars: and deduces from it, that a certain equalization, or approach to equality, may be obtained between the motion of the stars, or between that of the sun and any one of them selected for that purpose. These calculated velocities, he says, are such as would be true, if the stars were at the assumed distances, and if their real motions were performed in lines at right angles to the visual ray; but if the stars do not move in that direction, we should still certainly have the minimum of their velocities: and it must be allowed to be a considerable point gained if we could show what is likely to be the least velocity of the solar motion. Besides, if the velocities of any two stars are equalized when their motions are supposed to be perpendicular to the visual ray, they will be as much so when they make any other given angle with it; and it is the equalization, not the quantity of the velocities, that is required.

It is, Dr. Herschel says, evident, that either a certain mean rate, or a middle rank, should be assigned to the motion of the sun, unless very sufficient reasons should induce us to depart from this condition; and he thinks it most eligible to prefer the latter; but says, that nearly the same result will be obtained from either of the methods. He adds, that if we can at the same time bring the sidereal motions

to a greater equality among each other, it will be a very proper secondary consideration.

The necessary calculations for investigating the solar motion may, our author says, be divided into two classes: the first of these will remain unaltered, whatever may be the solar motion under examination; while the other must be adjusted to every change that may be required. The first will contain the angular quantity of the observed or apparent motion; its direction with the parallel of the star; its direction with the parallactic motion; and its velocity: the second, or changeable part, will consist of the angular quantity of the real motion; the parallactic direction of this motion; and its velocity.

A table containing the result of the calculations relating to the permanent quantities of 36 stars is now given: there are also various figures illustrating the said calculations: upon these, various remarks are made, which cannot be well understood without a view of the figures. We shall only observe, that several stars of the first magnitude appear to have less velocity than many which are much smaller. This, Dr. Herschel thinks, may be explained, if a solar motion is introduced; as the parallax arising from that cause will completely account for such a singular circumstance. He adds, that if the foregoing argument proves the expediency of admitting a solar motion, the direction of that motion is no less evidently pointed out to be in opposition to the motion of Arcturus.

By equalizing the velocities of the sun and α Orionis, the solar motion appears to be 1″·266. On the other hand, by a similar calculation of the velocities of Pollux and the Sun, those velocities will be equalized by a solar motion of 0″·967. These seem to be the limits of the solar motion, upon the supposition of its holding a middle rank among the sidereal velocities; and, by a mean of them, we may have the rank of the solar motion true to less than 0″·15. Upon this supposition, a table of the changeable quantities above mentioned is given, and also figures illustrating them.

Dr. Herschel, after observing that, if we except only ten of the above-mentioned stars, all the rest appear to be actuated by the same influence, and, like the sun, to direct their motions towards the same part of the heavens, proceeds to examine the causes of this marked singularity in their direction; which, he says, may arise either from their mutual gravitation towards each other, or from an original projectile force impressed upon them. As both these causes are known to act on all the bodies belonging to the solar system, they may reasonably be supposed to exert their influence likewise on the stars: and that this is really the case, our author endeavours to show by various arguments, which our limits necessarily oblige us to omit. He shows that the motions of the stars already mentioned cannot be accounted for by the mutual gravitation of neighbouring stars towards each other, or by a periodical binal revolution of them about a centre of gravity; but that we ought rather to have recourse to some far distant centre of attraction; which centre may

be either a single body of great magnitude; or it may be produced by the joint attraction of a great number of stars united into one condensed group; or, lastly, it may be formed by the union of several groups, which, he says, will create a still more powerful centre of gravitation.

Dr. Herschel now proceeds to the determination of the quantity and velocity of the solar motion: and, calculating upon the principles laid down in the course of the present paper, assuming, as we have already stated, that the solar motion holds a middle rank among the sidereal velocities, it appears that we have sufficient reason to fix upon the quantities of the solar motion to be such as, by an eye placed at right angles to its direction, and at the distance of Sirius from us, would be seen to describe annually an arch of 1·116992 of a degree; and its velocity, till we are acquainted with the real distance of the fore-mentioned star, can therefore only be expressed by the proportional number of 1116992.

Before he concludes, our author remarks, that the middle rank among the sidereal velocities, which he has assigned to the sun, agrees sufficiently with the phenomena that were to be explained. Thus the apparent velocities of Arcturus and Aldebaran, without a solar motion, are to each other as 208 to 12; but, according to the assumed solar motion, it appears, that when the deception arising from the parallactic effect is removed, these velocities are to each other only as 179 to 85, or 2 to 1. And although Arcturus still remains a star which moves with great velocity, yet it has been shown, in the eleventh table, that we have three or four stars with nearly as much motion, and five with more. The solar motion also removes the deception by which the motion of a star, of the consequence of α Orionis, is so concealed as hardly to show any velocity; whereas, by computation, we find that it really moves at a rate which is fully equal to the motion of the sun.

It will now be found, Dr. Herschel says, that we are within the reach of a link of the chain which connects the principles of the solar and sidereal motions with those that are the cause of orbitual ones: the probable motions of the sun and of the stars in orbits consequently becomes a subject that may receive the assistance of arguments supported by observations. And he further observes, that what he has said in a former paper, where the sun is placed among the insulated stars, does not contradict the present idea of its forming a part of a very extensive system. The insulation there ascribed to the sun relates merely to a supposed binary combination with some neighbouring star; and it has been already proved, by the example of Arcturus, that the solar motion cannot be occasioned, or accounted for, by a periodical revolution of the sun and the above, or any other star, about their common centre of gravity.

Observations upon the Marine Barometer, made during the Examination of the Coasts of New Holland and New South Wales, in the Years 1801, 1802, *and* 1803. *By* Matthew Flinders, *Esq. Commander of His Majesty's Ship* Investigator. *In a Letter to the Right Hon. Sir* Joseph Banks, *Bart. K.B. P.R.S.* &c. &c. &c. Read March 27, 1806. [*Phil. Trans.* 1806, *p.* 239.]

The chief circumstance that induced Capt. Flinders to think his observations upon the marine barometer were worthy of attention, was the coincidence that took place between the rising and falling of the mercury, and the setting in of winds that blew from the sea and from off the land, to which there seemed to be at least as much reference as to the strength of the wind or the state of the atmosphere.

Our author's examination of the coasts of New Holland and the other parts of the Terra Australis, began at Cape Leuwen, and continued eastward along the south coast. His observations, which, on account of their length, we must pass over, show, that a change of wind from the northern half of the compass to any point in the southern half, caused the mercury to rise; and that a contrary change caused it to fall. Also, that the mercury stood considerably higher when the wind came from the south side of east and west, than when, in similar weather, it came from the north side.

Capt. Flinders now proceeds to relate the observations he made upon the east coast. From these it appears that the winds which came from between south and east caused the mercury to rise and stand high, as the same winds had done, with only one exception, on the south coast. The wind from north-east kept the mercury up above thirty inches on the east coast, and caused it to rise after all other winds, except those from the south-east; whereas, on the south coast, those winds caused the mercury to fall, and to stand much below thirty inches; owing, in our author's opinion, to the wind coming from off the land. During north-west winds, the mercury stood lower upon both coasts than at any other time; and, on both, those winds came from off the land.

Moderate winds from the south-westward, with fine weather, caused a descent of the mercury on the east coast; and during their continuance, it was much lower than with winds from the north-eastward; but upon the south coast it rose with south-west winds, and it stood much higher than with winds from the opposite quarter. But it must be observed, that the wind which blew from the sea upon one coast came from off the land on the other.

The mean height of the mercury on the east coast is stated by our author to be not less than 30·08 or 30·10 inches; whereas upon the south coast he estimates its mean height to be 30 inches. The greatest range observed upon the east coast was from 29·60 to 30·36; while upon the south coast the range was from 29·42 to 30·51. But it must be remarked, that these extremes are taken for very short intervals of time.

The observations made by our author upon the north coast are next detailed. The chief differences in the effects of winds upon this coast, from what they produced upon the south and east coast, are, that a north-east wind raised the mercury as high, if not higher, than one from the south-east; and that a north-west wind, when it came from off the sea, and was moderate, was equal, in the above effect, to either of them, and kept the mercury higher than the south-west wind did.

Upon considering the effects of the same winds upon the different coasts of Australia, as described in the foregoing summary of Capt. Flinders's observations, the following queries seem, he says, to present themselves:

Why do the winds from the north and north-west, which cause the mercury to descend and stand lower than any other upon the south and east coasts, and also in the open sea, and in the south-west bight of the Gulf of Carpentaria, make it rise upon the outer part of the north coast with the same or even worse weather?

Why should the north-east wind, which occasions a fall in the barometer, upon the south coast, considerably below the mean standard, be attended with a rise above the mean upon the east and north coasts? The south-east wind, upon the south and east coasts, causes the mercury to rise higher than any other;—why has it not the same effect upon the north coast and upon the west?

How is it that the south-west wind, which makes the mercury rise and stand high upon the south and west coasts, causes it to fall below the mean standard upon the east coast, and, with the same weather, to descend lower than any other upon the north coast?

The answer to these questions Capt. Flinders considers as sufficiently obvious; in support of which opinion he offers the following explanation:

The lower air, when brought in by a wind from the sea, meets with resistance in passing over the land; and to overcome this resistance, it is obliged to rise and make itself room by forcing the superincumbent air upwards. The first body of air which thus comes in from the sea, being itself obstructed in its velocity, will obstruct the second; and this will therefore rise over the first, in like manner, to overcome the obstruction: and as the course of the second body of air will be more direct towards the top of the highest land it has to surmount than the first was, so the first part of the second body will arrive at the top before the latter part of the first body has reached it; and this latter part will not be able to pass over the top, being kept down by the second body and the successive stream of air, whose velocity is superior to it. In this manner an eddy or body of compressed air will be formed, which at first will occupy all the space below a line drawn from the shore to the top of the highest land; but the succeeding bodies of air, at a distance from the shore, will soon feel the effect of the obstruction, and will begin to rise; by which the stratum of lower air will be deeper between the top of the land and the shore, and to some distance from it, than upon the

mountains or in the open sea. Hence it follows, that the mercury ought to stand somewhat higher when such a wind blows than with the same wind when it meets with no obstruction; and the more direct it blows upon the coast, and the higher the land is, the higher ought the mercury to rise. On the other hand, when the wind comes from off the hills, this dense air will be displaced; and thus the air over the coast will resume its natural state with a land wind.

Capt. Flinders concludes his paper with some general remarks upon the barometer, of which the following seem to be the most material:

It is not so much the absolute as the relative height of the mercury, and its state of rising and falling, that are to be attended to in forming a judgement of the weather.

In the open sea, the changes in the weather, and in the strength of the wind, appear to be the causes that chiefly affect the barometer; but, near the shore, a change in the direction of the wind seems to affect it as much, or more, than either of those causes taken singly.

On the open sea, also, the mercury seems to stand higher in a steady breeze of several days' continuance, provided it does not blow hard, than when the wind is variable. Perhaps it is on this account, as well as from the direction of the wind, that the mercury stands higher within the tropics than in those parallels where the winds are variable.

Upon the whole, our author thinks the barometer capable of affording so much assistance to the commander of a ship, that no commander in a long voyage should be without one.

Account of a Discovery of native Minium. In a Letter from James Smithson, *Esq. F.R.S. to the Right Hon. Sir* Joseph Banks, *K.B. P.R.S.* Read April 24, 1806. [*Phil. Trans.* 1806, *p.* 267.]

The minium here described by Mr. Smithson was found disseminated in a compact carbonate of zinc. Its general appearance was pulverulent; but when a lens was used, it showed, in some places, a flaky and crystalline texture. Its colour was the same as that of factitious minium: when gently heated by the blowpipe it became more obscure, but returned, upon cooling, to its original colour. By a stronger heat it melted into litharge; and, upon charcoal, was reduced to lead.

In dilute nitric acid it assumed a coffee-colour; and on the addition of a little sugar, this brown calx was dissolved, producing a colourless solution. Upon being put into muriatic acid, with a little leaf-gold, the gold was soon entirely dissolved.

When it was inclosed in a small bottle with muriatic acid, and a small piece of turnsole paper was fixed to the cork, the paper in a short time entirely lost its blue colour, and became white. Even a slip of common blue paper, whose colouring matter is indigo, when placed in the above situation, underwent the same change.

This native minium, Mr. Smithson says, seems to be produced by

the decay of galena, which he suspects to be itself a secondary production, arising from the metallization of white carbonate of lead by hepatic gas. This, our author says, appears evident in a specimen which he means to send to Mr. Greville. In one part of this specimen there is a cluster of large crystals; one of which, upon being broken, was found to be converted into minium, to a considerable thickness, while its centre was still galena.

Mr. Smithson does not say where this native minium was found; but his letter is dated from Cassell in Hesse.

(Fistulana clava)

Description of a rare Species of Worm Shells, discovered at an Island lying off the North-west Coast of the Island of Sumatra, in the East Indies. By J. Griffiths, *Esq. Communicated by the Right Hon. Sir* Joseph Banks, *K.B. P.R.S.* Read February 13, 1806. [*Phil. Trans.* 1806, *p.* 269.]

The shells here described were discovered in a small island called Battoo, after a violent earthquake that occurred in the year 1797. Upon the receding of the inundation caused by the earthquake, they were seen protruding from a bank of slightly-indurated mud, in a small sheltered bay, surrounded by coral reefs. They were procured by means of a servant sent by Mr. Griffiths for that purpose, who was very expert in diving, and who stated that he found these shells sticking out of the mud to the extent of ten inches, or even more, and being from one to three fathoms under water. They were in considerable number, standing in different directions, and separate from each other. Mr. Griffiths was informed that the shells were filled with a soft gelatinous flesh, and that the animal threw out tentacula, resembling small Actiniæ, from the two apertures at the apex. They were easily extracted from their bed, but were all mutilated more or less: this, the author thinks, was occasioned by the earthquake.

The longest of these shells that came into the possession of Mr. Griffiths was five feet four inches in length. The circumference at the base was nine inches, tapering upwards to two inches and a half. But others were of very different dimensions. The large end of the shell is completely closed, and has a rounded appearance: at this part it is very thin. The small end, or apex, is very brittle, and is divided by a longitudinal septum, which extends downwards eight or ten inches, into two distinct tubes, from whence protrude the tentacula already mentioned. The substance of the shell has a radiated appearance, and having an outer crust of a pure white colour, and an inside enamel of a yellow tinge. The external surface is often interrupted by a sudden increase of thickness, which probably indicates the different growths of the shells; but these interruptions are merely on the outside shell, and do not extend into the radiated substance. The thickness of these shells varies very much; so also does their shape, some being nearly straight, others crooked and contorted. Their internal surface is generally smooth, but is sometimes

covered with excrescences resembling tubercles. There was no indication that the animal had adhered to any part of the shell.

The great length and size of these shells, and the division in the upper part, constitute, in Mr. Griffiths's opinion, their chief peculiarities. The radiated appearance of the substance of the shells is such, that they might, in his opinion, be easily mistaken for stalactites. Mr. Griffiths at first considered these shells as a new genus; but afterwards, on consulting the works of Rumphius, he found in that author a description of some shells, very similar, but differing by having two long-jointed tubes issuing from their upper part. These shells were found in shallow water among mangrove-trees.

Observations on the Shell of the Sea Worm found on the Coast of Sumatra, proving it to belong to a Species of Teredo; *with an Account of the Anatomy of the* Teredo Navalis. *By* Everard Home, *Esq. F.R.S.* Read May 1, 1806. [*Phil. Trans.* 1806, *p.* 276.]

In the first part of this paper Mr. Home relates some further particulars respecting the sea worm shell from Sumatra, of which an account was, some time since, laid before this Society by Mr. Griffiths. A specimen of one of these shells, five feet long, but imperfect at both ends, was given to Mr. Home by Capt. Maxwell; and in order to remove all doubt respecting its nature, a part of it was analysed by Mr. Hatchett, who found that it was composed of carbonate of lime and an animal gelatinous substance, greater in quantity than in the *Chama Gigas*, but less than in the common oyster.

The subsequent discovery of two boring shells and two flattened opercula, sufficiently evinced that the shell here treated of belonged to the genus Teredo; and as the internal structure and economy of Teredines are very little known, our author thought that nothing would tend more to enable us to form an adequate idea respecting this new species (which he thinks may be called *Teredo gigantea*), than an accurate knowledge of the common species *Teredo Navalis*. With this view, by the assistance of Sir Joseph Banks and Mr. Whitbey, Mr. Home obtained some pieces of wood, with live Teredines in them, from Sheerness. By means of these, and of some specimens in the British and the Hunterian Museums, he has been enabled, with the assistance of Mr. Clift and Mr. Brodie, to give a very circumstantial description, accompanied by drawings, of the anatomy of the *Teredo Navalis*.

The Teredines brought from Sheerness, lived in salt water for the space of three days after being brought to town, during which time these animals were observed to throw out two small tubes; the largest of which was about three fourths of an inch in length, and had, within its external orifice, a fringe composed of about twenty very small tentacula. These tentacula were visible only when the tube was fully extended, because the animal drew in this tube by inverting it; whereas the smaller tube was not inverted when drawn in. The smallest of these tubes appeared to be the most sensible; for the

larger one did not always retract when touched, but upon touching the smaller one, both were instantly drawn in.

In examining the shell while in the wood, there appeared to be a small portion, nearly at right angles to the cylinder, sufficient only to give a passage to the two small tubes. The shell was found, when analysed by Mr. Hatchett, to be perfectly similar to that of the *Teredo gigantea*.

The largest of these worms was 8 inches in length; many of them lived 24 hours after separation from the shell; in these the heart was distinctly seen to palpitate. The blood in the vessels going to the head, and also in the parts near the liver, was of a red colour; but this colour disappeared soon after death.

From the middle of the exposed part of the head proceeds a kind of proboscis. As this has no orifice in it, Mr. Home thinks it probable that it adheres to the wood, and acts as a centre-bit, while the animal works with the boring shells, between which it is situated. The mouth is nearly concealed by the projection of this proboscis. The body of the animal terminates in a small double fold, forming a cup, on the inside of which are two opercula, which, when brought together, close up the shell: these opercula do not correspond to the tubes, but are in a contrary direction. In the *Teredo gigantea* the opercula are situated in a similar manner, each shutting up one half of the bifurcation.

Some other observations, which our limits oblige us to omit, are made on these animals before Mr. Home proceeds to describe their internal structure as observed in the dissection. Here also we must content ourselves with mentioning the most striking circumstances, referring those who wish for more particular information to the paper itself.

Into the cavity of the worm there are two natural openings; one of these is the larger tube already described, by which it receives water from the sea; the other is an aperture under the boring shells, forming a slit in a transverse direction, which opens into the space before the boring shells.

The breathing organs are attached on the posterior surface of this cavity, and have their fringed edges loose, and exposed to the sea water, which passes through this cavity to the head of the animal. In the worms, which were examined while alive, the stomachs, which extend the whole length of the abdomen, were quite empty; but in the stomachs of some of the preserved specimens there was found a yellow-coloured pulp, which, from some experiments made on it by Mr. Hatchett, appeared to be an impalpable vegetable saw-dust. The intestine, after various inflections, terminates in the small tube, through which it empties its contents into the sea.

The heart consists of two auricles, which open into two tubes; these uniting, form the ventricle. The circulation is, of course, single; but the mode in which it is performed seems to be peculiar to this animal; the blood being thrown out from the heart, towards the viscera and the head, and carried afterwards through the ves-

sels of the breathing organs, from which it returns directly to the heart.

The mode in which the breathing organs of this animal are supplied with water, makes it evident, in Mr. Home's opinion, that all similar animals which have no cavity for the reception of sea water, must have their breathing organs placed externally; and he thinks that the beautiful membranous expansions displayed by those species of Actiniæ, called in the West Indies animal flowers, are, in fact, the breathing organs of those animals; and not, as their appearance formerly led Mr. Home to believe, tentacula for catching food.

The *Teredo gigantea*, when arrived at its full growth, closes up its shell; so also does the *Teredo Navalis*. Hence Sellius was led to suppose that the animal, by this act, formed its own tomb. This, however, is not the case; since, in some specimens in Mr. Griffiths's possession, the animal appears to have receded from its first inclosure, and to have formed a second, three inches up the tube, and afterwards a third, two inches further on. These facts show that the *Teredo gigantea*, when arrived at its full growth, closes up its shell, and lives a long time afterwards, being furnished with food from the sea by means of its tentacula. The *Teredo Navalis* closes up its shell in the same manner; it must therefore, after that period, be supplied with food through the medium of the sea water; and it is probable that the small tentacula, before described, are for the purpose of catching food.

As the *Teredo gigantea* bores in mud, from which it cannot be supposed to receive any part of its nutriment, it may be questioned whether the *Teredo Navalis* receives its support from the wood it destroys, or is wholly supplied with food from the sea. The latter opinion appears to Mr. Home the most probable. The quantity of wood taken into its stomach is, he thinks, by no means sufficient for the support of an animal which has red blood and very perfect organs. He also remarks, that the saw-dust already spoken of did not appear to Mr. Hatchett to have undergone any change.

These animals, having only a slight connexion with their shell at one particular spot, are capable of turning themselves round in their shell; this facility of motion seems evidently to be intended for the purpose of boring.

On the inverted Action of the alburnous Vessels of Trees. By Thomas Andrew Knight, *Esq. F.R.S. In a Letter to the Right Hon. Sir* Joseph Banks, *K.B. P.R.S.* Read May 15, 1806. [*Phil. Trans.* 1806, *p.* 293.]

Mr. Knight, in the papers formerly communicated by him to the Royal Society, endeavoured to prove that the fluid by which the various parts added to trees, &c. are generated, has previously circulated through their leaves, either in the same or in the preceding season, and has subsequently descended through their barks. There is, however, a circumstance stated by Hales and by Du Hamel, which

appears to militate against the above hypothesis, namely, that when two circular incisions are made, at a small distance from each other, through the bark, round the stem of a tree, and the bark between these incisions is wholly taken away; that portion of the stem which is below the incisions continues to live, and to increase in size, though much more slowly than the parts above the incisions. The above-mentioned naturalists have also observed, that a small elevated ridge is formed round the lower lip of the wound, which makes some slight advances to meet the bark and wood, projected in larger quantities from the upper lip of the wound.

Our author, in a former paper, attempted to explain the above circumstance, by supposing that a small part of the true sap, descending from the leaves, escapes downwards, through the porous substance of the alburnum: in another paper he has shown, from the growth of inverted cuttings, the existence of a power in the alburnum to carry the sap in different directions; and he now describes some experiments made in order to show that the conclusions drawn by him are not inconsistent with the facts stated by Hales and Du Hamel; and that although the ascending sap usually rises through the alburnum and central vessels, yet the alburnous vessels appear to be also capable of an inverted action, when such action becomes necessary to preserve the existence of the plant.

The first experiment described in the present paper, consisted in removing the bark by means of circular incisions at the distance of three inches from each other from the stems of several young oaks, as soon as the leaves were nearly full grown, and examining, in the succeeding winter, the state of the parts. In almost every instance the alburnum was found to be lifeless, and almost dry; in one instance, however, it was perfectly alive; and in this the specific gravity of the wood, above the decorticated space, was 1114, and below it 1111, whereas the specific gravity of an unmutilated stem, from the same roots, and at the same distance from the ground, was 1112. Now if the whole of the descending, or true sap, had in the above instance stagnated above the decorticated part, the specific gravity of the wood there ought to have been much greater than it was found to be.

Mr. Knight, conceiving that he should obtain more satisfactory and decisive results from tuberous-rooted plants, now proceeded to make some experiments on the potatoe. The early varieties of this plant, as is well known, afford neither blossoms nor seeds. This circumstance he attributed to the privation of nutriment, from the preternaturally early formation of the tubers; he therefore planted, in the last spring, some cuttings of a very early variety of the potatoe in garden-pots; and when the plants had grown a few inches high, they were secured to sticks, fixed erect in the pots. The mould was then washed away from the base of the stems, so that the plants were suspended in the air, and had no communication with the remaining soil, except by their fibrous roots. Efforts were soon made, by every plant, to produce runners and tuberous roots, but these were de-

stroyed as soon as they became perceptible. An increased luxuriance of growth now took place in all the plants; numerous blossoms were emitted, and every blossom afforded fruit.

In another experiment Mr. Knight, taking great care to prevent the formation of tubers on any other part of the plant, permitted them to form on the extremities of the lateral branches; these being the points most distant from the earth, in which the tubers are naturally deposited. Many of the joints of the plants became enlarged; and our author thinks, that if the formation of tubers had been totally prevented, these joints would have acquired an organization capable of affording plants in the succeeding spring.

In another variety of the potatoe, which was very luxuriant in lateral branches, Mr. Knight detached many of those branches from the principal stem, letting them, however, remain suspended by such a portion of alburnous and cortical fibres and vessels as was sufficient to preserve life. The result was, that the true sap, instead of returning down the principal stem into the ground, remained, and formed small tubers at the base of the leaves of the depending branches.

To ascertain whether the tubers would be fed when the passage of the true sap down the cortical vessels was interrupted, a portion of the bark, five lines in width, was removed from the stems of several potatoe plants, close to the surface of the ground, soon after the tubers had begun to be formed. The tubers continued to grow, but did not attain their natural size; partly, our author supposes, from the declining health of the plant, and partly from the stagnation of a portion of the true sap above the decorticated part.

The preceding experiments, Mr. Knight admits, do not prove that the fluid contained in the leaf passes downward through the decorticated space to be subsequently discharged into the bark below it; but he has, he says, found that if the amputated branches of different trees have their leaves immersed in water, a portion of that fluid will be absorbed, and will be carried downwards, by the alburnum, into the bark below a decorticated space; so that the insulated bark will be preserved alive and moist during several days. If the moisture absorbed by a leaf can be thus transferred, it appears very probable that the true sap will pass through the same channel. A considerable portion of that sap certainly stagnates above the wound, and a great part of that which escapes into the bark below the wound, is probably carried into the root. But some of that fluid will be carried upwards, by capillary attraction, and will stagnate on the lower lip of the wound, where, in Mr. Knight's opinion, it generates the small portion of wood and bark described by Hales and Du Hamel.

Our author concludes his paper by stating, that he has in his possession a piece of a fir-tree, from which a portion of bark, extending round its whole stem, had been taken off several years before the tree was felled. And he has ascertained that the specific gravity of the wood above the decorticated space is 0·590, that below it only 0·491; and having steeped pieces of each part, weighing 100 grains,

in water during twelve hours, he found that the latter had absorbed 69 grains, the former only 51. Hence he thinks considerable advantage may be expected from stripping off a portion of the bark from resinous trees, all round their trunks, close to the surface of the ground, in the beginning of the summer preceding the autumn in which they are to be felled. He even thinks it probable, that the timber would be improved by letting them stand a second year; although he admits that some loss would be sustained by the slow growth of the trees in the second summer.

It may, Mr. Knight says, be suspected, that the increased solidity of the fir-wood above described was confined to the part contiguous to the decorticated space; but it is well known that taking off a portion of bark round the branch of a fruit-tree, occasions in the succeeding season an increased quantity of blossoms on every part of that branch. This increase probably owes its existence to a stagnation of the true sap, extending to the extremities of the branch; and it may therefore be expected that the alburnous matter of the trunk and branches of a resinous tree will be rendered more solid by a similar operation.

A new Demonstration of the Binomial Theorem, when the Exponent is a positive or negative Fraction. By the Rev. Abram Robertson, *A.M. F.R.S. Savilian Professor of Geometry in the University of Oxford. In a Letter to* Davies Giddy, *Esq. F.R.S.* Read June 5, 1806. [*Phil. Trans.* 1806, *p.* 305.]

This paper is merely an extension of one formerly communicated to the Society by Mr. Robertson, and printed in the Philosophical Transactions for the year 1795. It is, the author says, so far as relates to the raising of integral powers, the same as that paper, and is confessedly new only to the extent mentioned in the title, namely, that the present demonstration is applicable when the exponent is a positive or a negative fraction. The nature of the paper is obviously such, as to render it unsusceptible of abridgement.

New Method of computing Logarithms. By Thomas Manning, *Esq. Communicated by the Right Hon. Sir* Joseph Banks, *K.B. P.R.S.* Read June 5, 1806. [*Phil. Trans.* 1806, *p.* 327.]

If, Mr. Manning observes, there existed as full and extensive logarithmic tables as ever will be wanted, and of whose accuracy we were absolutely certain, and if the evidence for that accuracy could remain unimpaired through all ages, then any new method of computing logarithms would be totally superfluous, so far as concerns the formation of tables, and could only be valuable indirectly, and inasmuch as it might show some curious and new views of mathematical truth. But the above kind of evidence is necessarily impaired by the lapse of time, even while the original record remains, and still more when the record must from time to time be renewed

by copies: nor is the uncertainty of copies being accurately taken greater in any case than in that of copied numbers. It is consequently useful to contrive new and easy methods for computing new tables, or for examining those we already have; and it is particularly useful to contrive methods by which any part of a table may be verified, independently of the rest; for by examining parts taken at random, we may, in some cases, acquire a moral certainty respecting the accuracy of the whole.

Among the various methods of computing logarithms, none, our author says, possesses the advantage of forming them with tolerable ease, independently of each other, by means of a few easy bases. This desideratum, he trusts, the method described by him will supply; being very easy of application, as it requires no division, multiplication, or extraction of roots, and has its relative advantages highly increased by increasing the number of decimal places to which the computation is carried.

The chief part of the work, according to the method laid down by Mr. Manning, consists in merely setting down a number, under itself, removed one or more places to the right, and subtracting; then repeating this operation. This method, consequently, is very little liable to error; and may be performed, after a few minutes instruction, by any one who is able to subtract. Besides, from the commodious situation of the figures, the work may be revised with great rapidity. It is as easy for large numbers as for small ones; and, on an average, about 27 subtractions will furnish a logarithm accurately to ten places of decimals.

A similar method, by addition only, may, Mr. Manning says, in some cases, be used with advantage; and various artifices may be occasionally employed to shorten the computation, both in the method by subtraction and in that by addition: the two methods may also be sometimes advantageously combined together. It must, however, be observed, that the method by subtraction has many advantages over that by addition; particularly as from its being more simple, and more completely mechanical, it may be confided to the most unskilful persons without much danger of error.

Various examples of our author's mode of computation and rules for conducting it are given, for which we must necessarily refer to the paper itself.

Description of the Mineral Basin in the Counties of Monmouth, Glamorgan, Brecon, Carmarthen, and Pembroke. By Mr. Edward Martin. *Communicated by the Right Hon.* C. F. Greville, *F.R.S.* Read May 22, 1806. [*Phil. Trans.* 1806, *p.* 342.]

The basin, which is here described by Mr. Martin, is delineated in a map annexed to the paper; it is formed of limestone, and contains all the strata of coal and iron ore in South Wales: it is upwards of 100 miles in length; and its average breadth in the counties of Monmouth, Glamorgan, Carmarthen, and part of Brecon, is

from 18 to 20 miles; but in Pembrokeshire its breadth is only from 3 to 5 miles.

On the northern half of the basin the strata rise gradually northward; on the south side they rise southward, except at the east end, where they rise eastward. The deepest part of the basin is between Neath, in Glamorganshire, and Llanelly in Carmarthenshire, where the depth of the principal strata of coal and iron ore is from 600 to 700 fathoms; whereas in Pembrokeshire, none of the strata lie above 80 or 100 fathoms deep.

The strata of coal at the east end of the basin and on the north side, are chiefly of a cokeing quality; but they alter, towards St. Bride's Bay, to what is called stone coal: on the south side of the basin the strata are principally of a bituminous or binding quality.

In this mineral basin there are 12 veins, or strata of coal, from 3 to 9 feet thick; and 11 others, from 18 inches to 3 feet, making in all 95 feet, besides a number of smaller veins, from 6 to 18 inches in thickness.

There are in these strata many faults or irregularities, by which the due range of the strata is thrown out of course. These faults are not confined to the edges of the strata, but run through the interior of the basin generally, in a north and south direction, and often throw the whole of the strata, for hundreds of acres together, 40, 60, 80, or 100 fathoms up or down. There is, however, seldom any superficial appearance that indicates a disjunction; for the greatest faults frequently lie under even surfaces.

A very considerable fault is observable at Crib-bath, where the beds, or strata of the limestone, stand erect. Another fault of great magnitude lies between Ystradvellte and Penderryn, where all the strata, and the north side of the basin, are moved many hundred yards southward.

The limestone appears at the surface, all along the boundary line, in the counties of Monmouth, Glamorgan, Carmarthen, and Brecon; and no doubt can be entertained that it ranges from Newton, across Swansea Bay, to the Mumbles, and from Canmaddock Hill, across Carmarthen Bay, to Langam Tenby. In Pembrokeshire it appears at the surface only in some particular spots; yet it certainly forms an under-ground connexion from one spot to the other.

Glamorganshire possesses by far the greatest portion of coal and iron ore; Monmouthshire is the next in point of quantity; then Carmarthenshire; then Pembrokeshire; and lastly Brecknockshire, which possesses the least.

Observations on the Permanency of the Variation of the Compass at Jamaica. In a Letter from Mr. James Robertson *to the Right Hon. Sir* Joseph Banks, *K.B. P.R.S. &c.* Read June 12, 1806. [*Phil. Trans.* 1806, *p.* 348.]

The object of Mr. Robertson, who resided in Jamaica, as a King's Surveyor of Land, upwards of twenty years, is to show that no

alteration has, for a considerable period, taken place there in the variation of the compass. In that island all grants of land have a diagram thereof annexed to the patent, which diagram is delineated from an actual survey of the land to be granted, and has a meridional line, according to the magnetical needle, laid down upon it; but no notice is taken of the true meridian. The boundary lines are marked upon the land; and in all disputes where the keeping up of these lines has been neglected, surveyors are appointed to make actual re-surveys, which are compared with those preserved in the secretary of the island's office; and it is expected that the lines and meridians of the former will coincide with those of the latter. It is evident, however, that this coincidence could not happen if any alteration in the variation had taken place in the interval between the two surveys. Mr. Robertson's business, as a surveyor, having been very extensive, he has had many opportunities of investigating the fact here treated of; and it appears from his observations, that the courses of the lines and meridians delineated on diagrams annexed to patents granted so long ago as the year 1660, coincide with, and are parallel to, the lines and meridians delineated on the re-surveys annexed to deeds, &c., or on the new diagrams, from recent surveys made by means of the magnetical needle, consequently no variation of the needle could have taken place, in Jamaica, during the above period of time.

Our author subjoins to his paper a short history of the practice of surveying in Jamaica, from the Restoration to the present time, in order to obviate any doubt whether the quantity of the magnetical variation was not ascertained and allowed for in the first diagrams annexed to patents; and whether the present variation of 6½ degrees east, might not then have agreed with the true meridian. He remarks, that until the year 1700, when Dr. Halley published his theory of the variation of the compass, no observations to ascertain the quantity of the variation in the West Indies had (so far as he knows) been published; and the variation at Jamaica, as laid down by Dr. Halley, appears to have been the same as it is at present. Besides, had the first surveyors allowed for the variation, in delineating their diagrams, they would not have omitted to mention it; and the same system of surveying would have been continued, since a difference of 6½° would have so totally deranged all boundaries, as to have demanded legislative interference and correction. But no instance of this kind has occurred.

In the maps of the island made by the direction of Sir Henry Moore, Governor (about the year 1760), the magnetic meridian only is represented, although that gentleman was considered a great surveyor. In short, the true meridian, our author says, has never been noticed, nor the quantity of variation ascertained, nor the latitude or longitude observed, by any surveyor or engineer in Jamaica except himself. He has ascertained the variation to be 6½° east, and has assumed that as the true quantity, in the maps lately published by him.

Observations on the Camel's Stomach respecting the Water it contains, and the Reservoirs, in which that Fluid is inclosed; with an Account of some Peculiarities in the Urine. By Everard Home, *Esq. F.R.S.* Read June 12, 1806. [*Phil. Trans.* 1806, *p.* 357.]

The camel, on which Mr. Home's observations were made, was a female, twenty-eight years old, and was brought from Arabia. It drank regularly, every second day, six gallons of water, sometimes seven and a half; but would not drink in the intervening period. It daily consumed a peck of oats, one of chaff, and one third of a truss of hay. It was killed on the first of April last, by thrusting a double-edged poniard between the skull and the first vertebra of the neck; it had a few hours before drank three gallons of water.

A very particular account of the animal's various stomachs, and of those of the bullock, together with the mode in which the food successively passes into them, is now given. From these (which our limits necessarily oblige us to omit,) it appears, that in the bullock there are three stomachs for the preparation of the food, and one for its digestion; whereas, in the camel, there is one stomach that answers the purpose of the two first of the bullock, a second employed merely as a reservoir for water, a third so small and simple in the structure of its internal membrane, that it can answer no purpose except that of retarding the progress of the food, and making it pass into the fourth stomach by small portions (for as it is not lined with a cuticle, it cannot be compared to any of the preparatory stomachs of the bullock), and a fourth, or true digesting stomach.

It appears, from our author's examination, that the camel, when it drinks, conducts the water in a pure state into the second stomach; that part of it is retained there, and the rest runs over into the first stomach, acquiring a yellow colour in its course. This purity of the water in the second stomach is a well-known fact; but by what means the water was kept separate from the food, had never been explained; nor had any other part been discovered by which the common offices of a second stomach could be performed. For Mr. Home's explanation of the mode in which the former is effected, we must refer to the paper itself, and especially to the figures of the parts with which it is accompanied.

From the facts stated by our author, the following gradation of ruminating stomachs is established by him.

Those ruminants which have horns, as the bullock, sheep, &c., have two preparatory stomachs for the food previous to rumination, and one for the food to be received in after rumination.

The ruminants that have no horns, as the camel, dromedary, llama, &c., have one preparatory stomach for the food before rumination, but none in which it can be properly said to be afterwards retained, before it passes into the digesting stomach.

Those animals that eat the same kind of food as the ruminants, but do not ruminate, as the horse and ass, have only one stomach; but a part of it is lined with a cuticle; in this part the food is first

deposited, and by remaining there some time, is more easily digested when it passes to the second, or digesting portion.

The difference, with respect to the teeth, between those animals which ruminate, and those which (although they eat nearly the same kind of food,) do not ruminate, is, according to our author, as follows.

Ruminants that are furnished with horns have molares, or grinding-teeth, in both jaws, and incisors only in the lower jaw.

Ruminants that are without horns have, besides the above, what may be called tusks, or fighting teeth; but these are of no use in eating. The Camelopardalis forms an intermediate link between the two, as it has short horns and no tusks.

Annexed to Mr. Home's paper, is an account of some experiments made on the urine of the Camel, by Mr. Brande.

The result of his experiments gives the component parts as follows; but it must be observed, that the quantity he had to operate upon was so small, that his analysis must be considered merely as an approximation to the truth.

Water	75
Phosphate of lime, muriate of ammonia, sulphate of potash, urate of potash	6
Muriate of potash	8
Urea	6
	95

Observations on the Variation, and on the Dip of the magnetic Needle, made at the Apartments of the Royal Society, between the Years 1786 *and* 1805 *inclusive. By Mr.* George Gilpin. *Communicated by* Henry Cavendish, *Esq. F.R.S.* Read June 19, 1806. [*Phil. Trans.* 1806, *p.* 385.]

The instruments with which Mr. Gilpin's observations on the variation, and on the dip of the magnetic needle were made, are the same as were used in former observations, and are described by Mr. Cavendish, in a paper printed in the 66th volume of the Philosophical Transactions. But as the observations now communicated by Mr. Gilpin are the first that have been given since the compass was put up in the Society's Apartments in Somerset Place, he has thought proper to describe very particularly its situation in the house, at the time the observations were made, and also the method he pursued to ascertain what allowances were proper to be made in the results of his observations.

After stating the circumstances above mentioned, Mr. Gilpin proceeds to the observations. These are detailed in five tables, of which the following is a summary account.

Table 1. contains, in sixteen pages, the observations made on the variation, at various but stated times of the day, from September 1, 1786, to December 31, 1787. It is so disposed, that the increase or decrease of the variation may be seen by mere inspection.

Table 2. contains the mean monthly variation for the times of the day stated in Table 1, during the above-mentioned period of sixteen months.

Table 3. contains the mean monthly true variation, and mean monthly diurnal alteration of variation during the above period; and also the mean monthly true variation, and mean monthly diurnal alteration of variation, for many months in each year, from 1786 to 1805 inclusive.

Table 4. contains the differences for twelve years (viz. from 1793 to 1805) between the observations made at the times of the vernal and autumnal equinoxes, and summer and winter solstices. From a mean of these, the variation appears to increase, or go westward, from the winter solstice to the vernal equinox $0'\cdot80$; and diminishes, or goes eastward, from the vernal equinox to the summer solstice $1'\cdot43$; it increases again, from the summer solstice to the autumnal equinox $2'\cdot43$; and continues nearly the same, only decreasing $0'\cdot14$ from the said equinox to the winter solstice.

Table 5. contains the dip of the magnetic needle from the year 1786 to 1805. During the first sixteen months the dip was observed as frequently as the variation; but as there did not appear to be any diurnal alteration in the dip, it has been thought sufficient to insert the mean for each month. From a comparison of Mr. Gilpin's observations in 1805 with those made by Mr. Cavendish in the year 1775, it appears that its mean annual decrease has been $4''\cdot3$; and its progressive annual decrease, in the above period, has been, on a mean, $1'\cdot4$.

On the Declinations of some of the principal fixed Stars; with a Description of an Astronomical Circle, and some Remarks on the Construction of Circular Instruments. By John Pond, *Esq. Communicated by* Smithson Tennant, *Esq. F.R.S.* Read June 26, 1806. [*Phil. Trans.* 1806, *p.* 420.]

The observations here given were made at Westbury, in Somersetshire, in the years 1800 and 1801, with a circular instrument of 2½ feet in diameter, constructed by Mr. Troughton. The stars selected by our author were, for a period of nearly two years, constantly observed on the meridian, when they passed at a convenient hour, generally reversing the instrument in azimuth at the end of every day's observation, and never making use of those observations that were not made in pairs.

Mr. Pond has subjoined to his observations a comparison with some procured for him by Mr. Troughton, in which he has included the latest observations made at Greenwich.

The deduced polar distances are also annexed to each observation.

Observations and Remarks on the Figure, the Climate, and the Atmosphere of Saturn, and its Ring. By William Herschel, *LL.D. F.R.S.* Read June 26, 1806. [*Phil. Trans.* 1806, *p.* 455.]

The observations made by Dr. Herschel last year, on the figure of Saturn, having drawn the attention of astronomers to the subject, a further investigation of it appeared to him to be necessary. Those, he says, who compare his former figures of the planet (in which the particular shape of the body was not meant to be represented,) with that annexed to his last paper, and wonder at the difference between them, have not attended to the measures he had given of the equatorial and polar diameters of it; these, as established in 1789, are the same as in his last figure, which differs only in having the flattening at the pole a little more extended on both sides; and as his attention, in 1789, was entirely taken up with an examination of the two principal diameters of the planet, it is not, he thinks, extraordinary that the singularity of its shape should then have been overlooked by him.

After some observations on the magnifying powers necessary to be used in observing the figure of Saturn, Dr. Herschel proceeds to relate the observations made by him, for that purpose, in the months of April, May, and the beginning of June, of the present year. He first, however, gives an observation made in the year 1788, from which it appears, that he had then observed the shape of Saturn not to be spheroidical (like that of Mars or Jupiter), but much flattened at the poles, and a little flattened at the equator.

The observations made this year by our author, agree with those made last year, in establishing, that the flattening at the poles of Saturn is more extensive than it is in Jupiter: also that the curvature in high latitudes is greater than in the last-mentioned planet; but, on the contrary, the curvature at the equator is rather less in Saturn than it is in Jupiter.

In the observation of May 16, of the present year, the greatest curvature in the disc of Saturn appeared to be at the latitude of about 40°.

Upon the whole, the figure of Saturn may, Dr. Herschel says, be called a spheroid, while that of Jupiter may be called an ellipsoid.

Our author now proceeds to notice some observations he has made on the periodical changes in the colour of the polar regions of Saturn. From those made in the years 1793, 1794, and 1796, when the south pole of the planet had been long exposed to the influence of the sun, it appeared that the regions about that pole had lost their former whiteness; and that the whiteness of the northern hemisphere was increased. Those made in the present year, when the north pole of Saturn is exposed to the sun, show that its regions have lost much of their brightness; while those about the south pole have regained their former colour, and are brighter, and whiter, than the equatorial parts.

Respecting the atmosphere of Saturn, Dr. Herschel observes, that

the brightness remaining on the north polar regions is not uniform, but is tinged with large dusky spaces, of a cloudy atmospheric appearance. From which, and the fore-mentioned changes of colour at the polar regions, added to the changes he has formerly observed in the belts, we have, he thinks, sufficient reason to infer the existence of a Saturnian atmosphere.

The Bakerian *Lecture, on some chemical Agencies of Electricity. By* Humphry Davy, *Esq. F.R.S. M.R.I.A.* Read November 20, 1806. [*Phil. Trans.* 1807, *p.* 1.]

The chemical effects produced by electricity have, Mr. Davy says, long been objects of attention; but the novelty of the phenomena, their want of analogy to known facts, and the apparent discordance of some of the results, involved the inquiry in obscurity.

It was very early observed, that acid and alkaline matter appeared in water acted upon by a current of electricity; but Mr. Davy soon found that the muriatic acid came from the animal or vegetable matters employed to connect the two portions of water; for when the same cotton was repeatedly used, it ceased to be evolved. The soda, in like manner, was found to proceed from the corrosion of the glass tube, as it did not appear in water electrified in an agate cup.

To be more certain of this effect, some distilled water was electrified in two agate cups, by the current from 150 four-inch plates, the communication between the cups being formed by moistened amianthus. In the first experiment soda was produced in the negative cup, but the quantity was much less than when glass tubes were used; and on repeating the experiment, its quantity decreased, so that in the fourth experiment the presence of soda was scarcely perceptible in the residual water. The water in the other tube was sour, and appeared to contain nitrous acid, with excess of nitrous gas. As similar effects were produced by electrifying water in small gold cones, Mr. Davy suspected that some minute portion of saline matter had been carried over during the distillation of the water; notwithstanding it did not affect nitrate of silver, or muriate of barytes. And on evaporating a quantity of it in a silver vessel by a heat not exceeding 140° Fahrenheit, a small residuum was actually left, which appeared to be a mixture of nitrate of soda with nitrate of lead. A portion of this residuum being added to water electrified in the usual manner, and which had attained the maximum of its effect upon turmeric paper, considerably increased those effects.

Water slowly distilled, being electrified either in gold cones or agate cups, did not evolve any fixed alkaline matter, though it exhibited signs of ammonia; but in tubes of wax, both soda and potash were evolved, and the acid matter in the positive cup was a mixture of sulphuric and muriatic acids. In a tube of resin the alkali was principally potash. In cups of Carrara marble, primitive marble from Donegal, argillaceous schist from Cornwall, serpentine from the Lizard, and grauwacke from North Wales, soda was uniformly evolved.

This probably arose from a minute portion of common salt being contained in them, for the Carrara marble yielded a sensible quantity on analysis; and on repeating the electrifying of the water in the cup formed of it, the presence of soda became less visible every time, and at length disappeared entirely; but the production of lime-water was uniform. A bit of glass added to water, which was electrifying in the gold cones, caused it to exhibit almost immediately the presence of soda.

In every instance nitrous acid was uniformly found in the positive cup, which appeared to proceed from the combination of nascent oxygen with the nitrogen of the common air absorbed by the water. The longer the operation was continued, the more acid was produced, arising from the air which continued to be absorbed. Volatile alkali was also constantly formed, from the combination of the nascent hydrogen with the nitrogen; but it soon attained its utmost limit, as hydrogen during its solution in water seems to expel nitrogen. When water was electrified in vacuo scarcely any nitrous acid, and no volatile alkali, was formed. When electrified in a receiver filled with hydrogen (the common air originally contained in the water having been extracted by the air pump), neither nitrous acid nor volatile alkali was found in the water.

In all these processes, the acid matter collected in the water round the point transmitting the electricity, and the alkaline matter round that which received it. When water was even electrified in two cups made of sulphate of lime, it was found that the water connected with the positive wire contained sulphuric acid, while that in the other cup was a pure and saturated solution of lime. Similar effects were produced when use was made of cups of sulphate of strontian, fluate of lime, or sulphate of barytes. It also appeared, that very minute portions of acid or alkaline matter might be disengaged by this means from solid combinations, consisting principally of the pure earths. When cups were used made of a basalt which contained 3½ parts of soda, and nearly half a part of muriatic acid, with fifteen parts of lime in the 100, oxymuriatic acid was found in the positive cup, and a mixture of lime and soda in the other. A cup of compact zeolite, containing seven per cent. of soda, yielded soda and lime to the water connected with the negative wire. Lepidolite yielded potash, and vitreous lava from Etna yielded a mixture of soda, potash, and lime.

Mr. Davy attempted to ascertain whether the weight of the alkali obtained, agreed with the weight lost by the substance operated upon. Water was electrified negatively for four days, by a current from 150 plates, in a glass tube that weighed 84 $\frac{48}{100}$ grains. The positive wire was inserted into water, contained in an agate cup, and the communication was kept up by moistened amianthus. At the end of the process the glass tube weighed 84 $\frac{47}{100}$ grains. The water being evaporated, yielded $\frac{47}{100}$ grains of a mixture of soda, with a white powder insoluble in acids.

When soluble compounds were put into water, contained in agate

cups, and electrified, the decomposition was more rapid. A solution of sulphate of potash being put into each of the cups, and electrified by means of fifty pair of plates for four hours, the acid was found by itself in the positive cup, and the alkaline bases in the negative cup. Similar phenomena took place with sulphate of soda, nitrate of potash, nitrate of barytes, sulphate of ammonia, and alum. When muriatic salts were used, these yielded oxymuriatic acid. When compatible mixtures of neutro-saline compounds were used, the different acids and bases separated in a mixed state, without any regard to their affinities. When solutions of metals, deoxidizable by nascent hydrogen were employed, metallic crystals formed on the negative wire, and some oxide was deposited; but solutions of iron, zinc, and tin, only deposited oxide; a great excess of acid was soon observed on the positive side. Although stronger solutions afforded signs of decomposition quicker than weaker ones, yet even the smallest proportions seemed to be acted upon with equal energy: as paper tinged with turmeric was immediately rendered brown when plunged into pure water and brought into contact with the negative point; so paper tinged with litmus was immediately reddened by the positive point, in consequence of the very minute portion of saline matter contained in the paper; and it further appeared, that in all these decompositions the separation of the constituent parts from the last portions of the compounds was complete when the operation was sufficiently protracted.

The contact of the solution with the wires was not necessary for its decomposition; for muriate of potash being put into the middle tube of a series of three, the outer ones containing only water and the wires, alkali soon appeared in that connected with the negative wire, and acid in the other; and at length they were obtained perfectly separate.

In thus causing the acids to be thus transferred from a saline compound into water, through moistened amianthus, no change was observed to take place in litmus paper placed near the amianthus. The reddening of the litmus paper always took place just above the positive point, and then slowly diffused itself to the middle of the vessel. Similar effects were observed when the alkali was transferred, the turmeric paper first becoming brown close to the negative wire.

When three glass tubes were used, the two outer tubes holding a solution of muriate of soda, and the middle one sulphate of silver, a communication being made with the central vessel by turmeric paper on the positive side, and by litmus paper on the negative, neither of the papers had its colour changed, although the muriatic acid passing through the amianthus occasioned a dense heavy precipitate in the sulphate of silver, and the soda a more diffuse and lighter one.

Acid or alkaline substances will also pass through liquids, having a strong attraction for them. In an apparatus of three tubes, Mr. Davy found that sulphuric acid, evolved from sulphate of potash, would pass into water, through either ammonia, lime-water, or weak solutions of potash or of soda. The only effect of strong solutions of

potash or soda was to increase the time necessary for this transfer. Muriatic or nitric acids were also transmitted through alkaline solutions; and, *vice versâ*, alkaline matter, by changing the arrangement of the liquids, was transmissible through the acids: even magnesia was transferred like the other bases. But when it was attempted to pass sulphuric acid through aqueous solutions of barytes or of strontian, on the contrary, the sulphate of those earths was deposited in the intermediate tube.

Muriate of barytes being positive, sulphate of potash intermediate, and water negative, potash appeared in the water, and sulphate of barytes was deposited in the intermediate vessel.

Muriate of barytes being negative, sulphate of silver intermediate, and water positive, sulphuric acid only appeared in the water, and muriate of silver was deposited in the intermediate vessel.

Mr. Davy then proceeds to develop the theory of these phenomena: and he first considers the mode of decomposition and transition. Hydrogen, alkaline substances, and metallic oxides, are attracted by negatively electrified metallic substances, and repelled by positive ones: contrary effects take place with oxygen and the acids. He thinks these electric energies are communicated from one particle to another of the same kind, so as to establish a conducting chain in the fluid, as acid matter is always found in the alkaline solutions through which it is transferred, as long as any acid matter remains at the original source. It is possible that there may be, in some cases, a succession of decompositions and recompositions; but the process is not absolutely necessary, as pure acids and alkalies pass through water to the points by which they are attracted. It appears that this power of transference is overcome by gravity in attempting to pass barytes through sulphuric acid, or muriatic acid through sulphate of silver. To these instances he adds, that magnesia and the metallic oxides will not pass through an intermediate vessel of water, but sink to the bottom.

In the decomposition of water, a particle of oxygen is attracted by the positive point, and at the same time a particle of hydrogen is repelled by it; the opposite process takes place at the negative point: and in the middle of the circuit there must be a new combination of the repelled matter, in the same manner as when two portions of muriate of soda are separated by water; muriatic is repelled from the negative side, and soda from the positive side; so that muriate of soda is composed in the middle vessel.

Although similar effects could not be produced by silently passing a strong current of electricity from an electrical machine for four hours, through sulphate of potash, yet they were produced by using platina wires $\frac{1}{70}$th of an inch in diameter, cemented in glass tubes; so that no doubt can arise but that the principle of action is the same in common as in voltaic electricity.

It was known that many bodies brought into contact and then separated, exhibited opposite states of electricity; and Mr. Davy himself had observed, that when acid and alkaline solutions were em-

ployed in alternation with plates of a single metal, the alkaline solution received the electricity from the metal, and the acid transmitted it to the metal. So in the simplest case of electrical action, the alkali, receiving electricity from the metal, would, on being separated from it, appear positive, and of course be repelled by positively electrified surfaces, and attracted by negatively electrified surfaces; the acid, acquiring the contrary electricity, following the contrary order.

Several phenomena accord with this idea. Sulphur separated from its contact with a metal is positive; and, in like manner, sulphur separated from sulphuric acid by electricity is attracted by the negative surface. And again, hydrogenated sulphuretted alkalies, being composed of three substances, all positive, are so very active in voltaic combinations as, in certain cases, to overpower the energies of the metals.

Mr. Davy then considers the relation between the electrical energy of bodies and their chemical affinities; and observes, that all the substances that combine chemically, exhibit, so far as is known, opposite electric states; and that, in the various experiments of decomposition by electricity, the natural electrical energies of the constituent parts of the compounds acted on are, as it were, overpowered by the artificial electricities: so that it is probable that chemical affinity depends upon the different electrical energies of the acting bodies.

And upon this supposition, particles possessed of opposite electrical states and freedom of motion will enter into combination. When two bodies possessed of the same state act upon a third body possessed of the opposite state, the substance possessing the weakest energy will be repelled. In other cases there may be such a balance of attractive and repellent powers as to produce triple compounds, or even more complicated combinations. It will also be easy to explain the influence of the masses of bodies upon their affinities; for the combined effect of many particles possessing a feeble electrical energy may be conceived equal, or even superior, to the effect of a few particles possessing a strong electrical energy; and, accordingly, we find that concentrated alkalies resist the transmission of acids more powerfully than weak ones.

As the strength of the electricity diminishes from the points to the middle of the water, a measure of the artificial energies may be obtained by the place where the compounds begin to be decomposed. Thus sulphate of barytes requires intermediate contact with the wires; and when 150 pieces of plates were used with a circuit of water of ten inches, sulphate of potash was not decomposed at four inches from the positive wire; but when brought within two inches, its alkali was strongly repelled.

As heat and light are the consequences of a restoration of the equilibrium between bodies in a high state of opposite electricities, so are they also the result of all intense chemical action; and again, as when large quantities of electricity of low intensity act, heat is produced without light, so in low combinations the temperature only is increased. The effect of heat in promoting chemical action seems not

confined to the freedom of motion it gives to the particles, but to the exaltation of the electrical energies of bodies, such as is well known to take place in glass, tourmalin, and sulphur. The electricity of an insulated plate of copper, and a plate of sulphur, was scarcely sensible at 56° Fahr. to the condensing electrometer; at 100° they affected the gold leaves without condensation; they increased in a still higher ratio as the sulphur approached its melting point; and at a little above that point, the two substances rapidly combine, and evolve heat and light. In general, when the different electrical energies are equally strong, the combination seems to be vivid, the heat and light intense, and the new compound is in a neutral state, as in combustion, and the union of the strong acids with the alkalies; but when only the energy is strong, the effect is less vivid, and the compound exhibits the excess of the stronger energy.

Mr. Davy then considers the theory of the Voltaic apparatus; and remarks, that the above facts seem to reconcile Volta's own theory with the chemical theory of galvanism.

In a pile of zinc, copper, and water, the plates are in opposite states of electricity; and in regard to such low electricity, the water is an insulating body. Of course, each plate produces an opposite electricity upon the opposite plate, the intensity increasing with the number, and the quantity with the extent of the series. Upon connecting the extreme points, the opposite electricities tend to produce an equilibrium, which would cause the motion to cease: but the fluid medium being composed of two elements possessing opposite electrical energies, the oxygen is attracted by the zinc, and the hydrogen by the copper. Hence the balance of power is only momentary; for oxide of zinc is formed, and hydrogen is disengaged. The electricity acquired by the copper is communicated anew to the zinc, and the process continues as long as the chemical changes are carried on.

Neither the conducting nor solvent powers of the fluid mediums are a principal cause of their activity; for strong sulphuric acid has very little activity in the pile. The effect of neutro-saline solutions diminishes when troughs are used, according as their acid arranges itself on the side of the zinc plates, and their alkali on the other; but the energy may be restored by agitating the fluids in the cells.

As sensible heat appears when an equilibrium is produced by small metallic surfaces in the voltaic battery, the opposite states being exalted, so if the decomposition of the chemical agents be essential to the balance of the opposed electricities, the decomposition of the saline solutions ought to be attended with an increase of temperature. To water electrified in the gold cones by the power of 100 plates, a drop of solution of potash was added in the positive cone: potash immediately passed over, and in less than two minutes the water was in a state of ebullition. When solution of nitrate of ammonia was employed, the water was evaporated in three or four minutes with a kind of explosive noise, and inflammation took place. The pure alka-

lies, or acids, produced very little effect, although they are better conductors.

Lastly, some general applications of these singular facts are pointed out by Mr. Davy; such, for instance, as the application of electricity to the analysis of animal and vegetable substances. Muscular fibre being electrified for some time, became dry and hard; and it left, upon incineration, no saline matter. Potash, soda, ammonia, lime, and oxide of iron, were collected on the negative side; and the sulphuric, nitric, muriatic, phosphoric, and acetous acids, on the positive. Laurel-leaf was rendered brown and parched; green colouring matter, with resin, alkali, and lime, appeared in the negative vessel, and prussic acid in the positive.

By using charcoal and plumbago, or charcoal and iron, as the exciting powers, along with neutro-saline solutions, large quantities of acids and alkalies might be procured with little trouble or expense.

It is very probable that many mineral formations have been materially influenced or even occasioned by the agency of the electricity; and the electrical power of transference may thus be applied to the explanation of the principal points in geology.

On the Precession of the Equinoxes. By the Rev. Abram Robertson, *M.A. F.R.S. Savilian Professor of Geometry in the University of Oxford.* Read December 18, 1806. [*Phil. Trans.* 1807, *p.* 57.]

The Professor observes, that Sir Isaac Newton was the first mathematician who endeavoured to estimate the quantity of the precession from the attractive influence of the sun and moon on the spheroidal figure of the earth. His investigations relating to this subject evince the same transcendent abilities that are displayed in other parts of his Principia; but it is admitted, that, from a mistake in his process, his conclusion is erroneous.

The investigations of other mathematicians in attempting the solution of the same problem are arranged by the author under three general heads. The first arrive at wrong conclusions, in consequence of mistake in some part of their proceedings; the second obtain just conclusions, but rendered so by balance of opposite errors; the third approach as near the truth as the nature of the subject will admit, but, in the author's estimation, are liable to the charge of obscurity and perplexity.

The defects in all their investigations Professor Robertson ascribes to the same cause—the uncultivated state of the doctrine of compound rotatory motion.

The author's endeavours are, consequently, first directed to the investigation of the principles of compound rotatory motion from principles which he considers clear and unexceptionable.

He next proceeds to calculate the disturbing solar force on the spheroidal figure of the earth, and thence the angular velocity which it produces.

The quantity of annual precession is then computed; and, lastly,

that of nutation, as far as these are produced by the disturbing force of the sun.

A similar deviation, the author observes, is produced by the action of the moon; but a minute investigation of the deviation from that cause is foreign to the design of the present communication.

An Account of two Children born with Cataracts in their Eyes, to show that their Sight was obscured in very different Degrees; with Experiments to determine the proportional Knowledge of Objects acquired by them immediately after the Cataracts were removed. By Everard Home, *Esq. F.R.S.* Read January 15, 1807. [*Phil. Trans.* 1807, *p.* 83.]

The design of the present communication is to explain a disagreement between the results of former experiments on this subject; since a very intelligent boy, thirteen years of age, couched by Mr. Cheselden in 1728, was unable, upon receiving his sight, to distinguish the outline of any object placed before him, and thought that everything before him touched his eye; but the cases communicated to this Society by Mr. Ware in 1801 lead to a different conclusion.

The subjects of Mr. Home's experiments were not equally sensible of light previous to the operation of couching, and consequently were not similarly affected by objects presented to them after receiving their sight.

The first was a lad twelve years of age, with cataracts in both eyes, that appeared to have existed from the time of his birth, as it had been noticed from his earliest infancy that his eyes rolled about in an unusual manner, and were not directed to objects before him, nor were his hands ever stretched out to catch at anything; but it was not till the child was six months old that his mother examined his eyes with attention, and observed cataracts as distinct as when he was brought to Mr. Home.

Previous to the operation, this boy could distinguish light from darkness, and the light of the sun from that of a candle, saying, it was redder and more pleasant to look at; but lightning made a still stronger impression. He was accustomed to call all light red. He had some conception of size, and said the sun was the size of his hat, and that the flame of the candle was larger than his finger, but less than his arm. He directed both his eyes at once to these objects; and when a candle was nearer than twelve inches he said it touched him, but at twenty-two inches it was invisible.

The operation of extracting the crystalline lens was first performed on the left eye; but as the cataract was in this instance found to be fluid, and the inflammation which followed was considerable, the operation of couching was preferred to it for the right eye, and was performed after an interval of eight weeks.

After the first operation, the eye was so imperfect in its powers, the pupil so contracted, and the surface of the cornea so irregular, that he could not discern any object distinctly, but imagined that

everything presented to him touched his eye, and still continued to call every object red.

After couching, the inflammation which followed was not so considerable as in the former case; but this cataract was also fluid, and, from its diffusion through the aqueous humour, occasioned a temporary opacity, which concealed for a time an opacity of the capsule, which rendered the sight permanently imperfect.

It was not till the end of four weeks from the second operation that the sight was again examined, when it was found that he had acquired some knowledge of colours and of the distances of objects, which no longer appeared to touch his eye; but he could not be said to have acquired any discernment of figure.

The second opportunity which Mr. Home had of making similar experiments was in a boy of seven years of age, who had been blind from his birth by cataracts in both eyes. His blindness, however, was not so complete as in the preceding case: his pupil contracted considerably when a lighted candle was placed before it, and by daylight he could distinguish many colours with tolerable accuracy, particularly the more vivid ones. The operation of couching was again preferred in this case, with the hope of avoiding inflammation. It gave very little pain, and was attended with the desired success.

As it was hoped that the eye would be but little disturbed by the operation, Mr. Home was prepared to examine his perception of objects as soon as the operation was over. After the eye had been allowed ten minutes to recover itself, a round piece of card of a yellow colour was placed about six inches from it. He immediately said that it was yellow; but upon being asked the shape, he expressed a wish to touch it. Upon this being refused, he continued to look at it, and at length guessed that it was round; and when a square blue card was put before him, he said that this was blue, and round also. A triangular piece he also called round. With regard to the distance of objects, he could form no judgement, but did not even at first suppose that any of them touched his eye.

The pleasure which this boy received from receiving his sight was such, that it was found impossible to restrain him from using it. The experiments were consequently repeated, and carried further at the distance of about two hours from the operation. Upon being desired to examine a square card with attention, he after some time said, he had found a corner, and then readily counted four corners; and in the same manner counted three angles of a triangle, by running his eye along the edges from corner to corner. He was on various occasions deceived, as might be expected, by the different apparent magnitudes of the same body at different distances: a guinea at fifteen inches distance was taken for a seven shilling piece; but at five inches he thought it a guinea. Upon seeing a cart from a two pair of stairs window, he took it for a wheelbarrow drawn by a dog, and counted the number of horses in other carts, supposing them to be dogs. At the end of a fortnight, when the different pieces of card were placed before him, he could not immediately tell

their shape. These being objects which he had not been accustomed even to feel, he was still learning them as a child learns to read: he could distinguish the angles, and could count their number in succession; but at the expiration of the third week, he could tell these forms nearly as readily as their colour.

The inferences which Mr. Home draws from these, are, that when the eye, before the cataract is removed, has only been capable of discerning light, without any power of distinguishing colours, then objects, after its removal, appear to touch the eye, and there is no knowledge of their outline, agreeably to the observations made by Mr. Cheselden. But when the eye has previously been able to distinguish colours, it has then also some knowledge of distances, though not of outline, but will soon attain this also, as happened in Mr. Ware's cases.

In a practical view, these cases confirm what has been laid down by Mr. Pott and by Mr. Ware, with regard to cataracts, in being generally soft, and in recommendation of couching as the operation which is best adapted for removing them.

Observations on the Structure of the different Cavities which constitute the Stomach of the Whale, compared with those of ruminating Animals, with a View to ascertain the Situation of the digestive Organ. By Everard Home, *Esq. F.R.S.* Read February 12, 1807. [*Phil. Trans.* 1807, *p.* 93.]

Mr. Home, having in a former paper communicated his observations upon the stomachs of ruminating animals, gives the present account of that organ in the whale tribe, to show that it forms a link in the gradation towards the stomach of truly carnivorous animals.

The *Delphinus delphis* of Linnæus, the bottle-nosed porpoise, called by Mr. Hunter the bottle-nosed whale, having been brought ashore alive by some fishermen at Worthing, Mr. Home took the opportunity of examining the structure of its stomach, and discovered a resemblance between the second, third, and fourth cavities in the whale, and the different parts of the fourth cavity in the camel and bullock, which appeared to throw some light upon their uses, as well as upon digestion in general.

The œsophagus in this porpoise is very wide: it has a number of longitudinal folds, and is lined with a strong cuticle, which is continued throughout the first stomach. This stomach lies in the direction of the œsophagus, without any contraction to mark its origin, and bears a strong resemblance in shape to a Florence flask. The coats of its cavity are firm, and are surrounded by a strong muscular covering.

The orifice leading to the second stomach is at right angles to the first, and at a small distance only from the œsophagus: the canal from thence into the second stomach is three inches long, and opens into it by a projecting orifice two inches and a half in diameter, at which the cuticular covering of the preceding parts terminates.

This stomach is nearly spherical, seven inches in diameter, of a honeycombed appearance and glandular structure. The exit towards the third stomach is placed very near the entrance from the first, and is only five eighths of an inch in diameter.

The third cavity is also spherical, and two inches in diameter, with an aperture only three eighths of an inch in diameter, leading to a fourth stomach. This cavity is nearly cylindrical, like an intestine, but rather widest, measuring nearly three inches at its further extremity, and fourteen inches and a half in length.

The pylorus, which is the boundary of this stomach, is only one fourth of an inch in diameter. The dilated cavity into which this opens has been considered by Cuvier and Hunter as belonging also to the stomach; but Mr. Home observes, that it should rather be considered as duodenum, since the common duct of the liver and pancreas opens into it.

The common porpoise, the grampus, and piked whale, have also four cavities constituting the stomach; but in the bottle-nosed whale of Dale there are as many as six: the general structure, however, is the same; and in all the whale tribe there is but one cavity lined with a cuticle, as in the camel and bullock. In all of them the second cavity has a very glandular structure, and in all the third is very small. The fourth stomach also, in each of them, has a smooth internal surface, with orifices of glands opening into its cavity.

The first stomach appears not to be a mere reservoir, since the food undergoes a considerable change in it. The flesh is here entirely separated from the bones, of which several handfuls were found without the smallest remains of the fish to which they belonged; the orifices into the second and third stomachs being too small to admit the bones to pass. The bones must consequently be reduced to a jelly in the first stomach, but require a longer time for the completion of that process than the fleshy parts.

The second cavity is that which Mr. Hunter supposed to be the true digesting stomach; but Mr. Home, notwithstanding his deference for every opinion of Mr. Hunter's, is of a contrary opinion, from considering that any further cavities would in that case be superfluous, after the complete formation of chyle, and from observing that the last cavity is that which, in its structure, bears the closest analogy to the simple human stomach, in which the process of forming chyle is certainly completed. From a comparison also of these stomachs with the fourth of the camel, it appeared that only the lower portion of that cavity is the stomach, in which the chyle is formed, and that its upper or plicated portion serves only to prepare the food for the process of digestion. In the same manner also in the bullock, although there is not the slightest contraction or subdivision between the upper and lower portions, Mr. Home considers the plicated part as a mere preparatory organ, and the lower as that which secretes the proper gastric juice.

As the stomachs of the camel, bullock, and horse, form principal links in the gradation from the most complex ruminating stomachs

to the most simple for digesting *vegetable* food, so those of the bullock, camel, and whale, are links from the ruminating to the most simple stomachs for digesting animal food; and the camel's stomach is the most important link in each series, the contraction peculiar to its fourth cavity making it intermediate between the bullock and the whale.

Although the above facts appear to throw some light on the digestion of different kinds of food, they also present difficulties which must remain to be explained when further progress has been made in the investigation. It is in general admitted, that animal substances do not require so long a process to convert them into chyle as vegetables; and hence the stomachs of carnivorous animals are in general most simple: but why the whale tribe, which live on fish that are very readily converted into chyle, should have a more complex stomach, it is not easy to explain. What further uses, in regard to other secretions, these preparatory stomachs may have, are foreign to the design of the present paper, which Mr. Home considers as a continuation of a series of observations on digestion, and hopes to extend further at some future opportunity.

On the Formation of the Bark of Trees. In a Letter from Thomas Andrew Knight, *Esq. F.R.S. to the Right Hon. Sir* Joseph Banks, *K.B. P.R.S. &c.* Read February 19, 1807. [*Phil. Trans.* 1807, *p.* 103.]

An extraordinary diversity of opinion having prevailed amongst naturalists most capable of correct observation, respecting the production and subsequent state of the bark of trees, Mr. Knight has undertaken to investigate the subject: but such are the difficulties of the subject, that, in a course of experiment which has occupied more than twenty years, he has scarcely felt himself prepared, till the present time, even to give an opinion of the manner in which the cortical substance is either generated in the ordinary course of its growth, or re-produced when that which previously existed has been taken off.

Du Hamel had shown, that the bark of some species of trees is readily re-produced when the decorticated alburnum is secluded from the air. Mr. Knight has repeated these experiments on the apple, the sycamore, and some other trees, with the same result; and has also observed, that the wych-elm, in moist and shady situations, will frequently re-produce its bark when no covering whatever has been applied.

A glairy fluid (as Du Hamel justly observes) exudes from the surface of the alburnum, which appears to change into a pulpous organized mass, and subsequently becomes organized and cellular,—facts which are extremely favourable to the opinion of Hales, that the bark is derived from the substance of the alburnum. But other facts may be adduced which lead to a contrary conclusion; since the internal surface of pieces of bark, when detached from contact with the alburnum, but remaining united to the tree at their upper

ends, will much more readily generate a new bark than the alburnum does under similar circumstances. A similar fluid exudes, and the same phenomena are observable, in both cases. The cellular substance, however, which is thus generated is for some time very imperfectly organized, since the vessels which carry the true sap are at first wanting; but Mr. Knight has been enabled to trace their progress, and, by appropriate management, to direct the course of their growth laterally or downwards with a greater or less degree of inclination; but observes, that their natural tendency is perpendicularly downwards, and that they cannot be made to extend themselves upwards excepting to a very short distance.

Mr. Knight having thus satisfied himself that both the alburnum and the bark of trees are capable of generating new bark, or at least of transmitting a fluid to which the bark owes its existence, his attention was next directed to discover the sources from which this fluid is derived.

Both the bark and the alburnum, he observes, are composed principally of two substances, one of which consists of long tubes, the other is cellular; and the cellular substance of the bark is in contact with a similar substance in the alburnum. Mr. Knight has shown, in former communications, that the true sap of trees acquires those properties which distinguish it from the fluid recently absorbed, by circulating through the leaf; that it descends down the bark, where part of it is employed in generating the new substance annually added to the tree; and that the remainder, not thus expended, passes into the alburnum, and there joins the ascending current of sap.

In the preceding experiments the cellular substance, both of the bark and alburnum, has been proved to be capable of affording the sap a passage through it; and it therefore appears not very improbable that it executes an office similar to that of anastomosing vessels in the animal economy, when the cellular surfaces of the bark and alburnum are in contact with each other; and hence the passing fluid is capable of exuding from both surfaces when they are detached; because almost all the vessels of trees are capable of an inverted action in giving motion to the fluids they contain.

Since the power of generating new bark appeared to reside alike in the sap of the bark and in that of the alburnum, Mr. Knight endeavoured to ascertain whether the fluid which ascends through the central vessels of the succulent annual shoot possesses similar power. For this purpose he removed the bark between two circular incisions round several annual shoots of the vine; and having taken care to prevent the formation of new bark on the surface of the alburnum, he, after a day or two, made longitudinal incisions through the dry and lifeless alburnum. These incisions commenced a little above and extended a little below the decorticated spaces. A cellular substance was in consequence generated through the whole length of the incision, and a perfect vascular bark was subsequently formed, and appeared to execute the office of that which had been removed, although placed beneath the alburnum.

The medulla in this case appeared to be wholly inactive.

By an examination of buds which had been inserted in a preceding summer, or attached by grafting in the spring, it appeared that vessels diverged from them into the bark of the stock.

It seems therefore probable, that a pulpous organizable mass first derives its matter from the bark or alburnum, and that this matter subsequently forms the new layer of bark; for if the vessels had proceeded as radicles (according to Darwin's supposition,) from the inserted buds or grafts, such vessels would be different from the natural vessels of the stock; neither is it probable that vessels could extend, by actual addition to their extremities in the course of a few days, from the leaves to the roots. Mr. Knight is consequently inclined to believe that the matter itself which composes the new bark acquires an organization of vessels calculated to carry the true sap; but whether, in the ordinary growth of trees, this matter be derived from the bark or the alburnum, or from both, it will be extremely difficult to determine. This, however, is certain, that bark in many cases exists previous to the existence of alburnum; but there seems to be no case, in the ordinary growth of vegetables, in which alburnum exists previous to the formation of bark.

An Investigation of the general Term of an important Series in the inverse Method of finite Differences. By the Rev. John Brinkley, *D.D. F.R.S. and* Andrews *Professor of Astronomy in the University of Dublin. Communicated by the Astronomer Royal.* Read February 26, 1807. [*Phil. Trans.* 1807, *p.* 114.]

On Fairy-rings. By William Hyde Wollaston, *M.D. Sec. R.S.* Read March 12, 1807. [*Phil. Trans.* 1807, *p.* 133.]

In this paper the author relates briefly some observations which he formerly made on the progressive changes of these rings, which appear to him to lead to a satisfactory explanation of their origin.

In the first place he observed, that some species of fungi were always to be found at the exterior margin of the dark ring of grass if examined at the proper season. This position of the fungi led him to conjecture that progressive increase from a central point was the probable mode of formation of the ring; and he thought it likely that the soil which had once contributed to the support of fungi, might be so exhausted as to be rendered incapable of producing a second crop. The defect of nutriment on one side would occasion the new roots to extend themselves solely in the opposite direction, and would cause the circle of fungi continually to proceed, by annual enlargement, from the centre outwards. The luxuriance of the grass follows as a natural consequence, as the soil of an interior circle is enriched by the decayed roots of fungi of the succeeding year's growth. Such a progressive enlargement, he remarks, had already been observed by Dr. Hutton on the hill of Arthur's Seat near Edinburgh; but Dr. Hutton had not attended to the production of fungi.

Dr. Withering, on the contrary, remarked the connexion of the rings with fungi, but had not noticed their progressive enlargement.

During the growth of fungi, the author observes, they so entirely absorb all nutriment from the soil beneath, that the herbage is often for a while destroyed, and a ring appears bare of grass, surrounding the dark ring; but after the fungi have ceased to appear, the soil where they had grown becomes darker, and the grass soon vegetates again with peculiar vigour.

For the purpose of observing the progress of various circles, he marked them by incisions for three or four years in succession, and found their annual increase to vary from eight inches to as much as two feet, according to the species of fungus to which they are owing; for he has observed as many as five species that have this mode of growth;—*Agaricus campestris*, *Ag. orcades*, *Ag. procerus*, *Ag. terreus*, and the *Lycoperdon bovista*.

The author has had many opportunities of remarking, that when two circles interfere with each other's progress, they do not cross each other, but are invariably obliterated between the points of contact. The exhaustion occasioned by each obstructs the progress of the other, and both are starved; a circumstance which he considers as a strong confirmation of his hypothesis.

He has further remarked, in one instance, that different species of fungi appear to require the same nutriment: for, in a case of interference of a circle of mushrooms with another of puff-balls, the circles were, as in other cases, both obliterated between the points of union.

With the hope of ascertaining in what length of time a soil might recover the power of producing a fresh crop of fungi, a groove was cut along the diameter of a mushroom-ring, and a quantity of the spawn taken from its circumference was inserted along it; but the experiment unfortunately failed altogether, and the author had no opportunity of repeating the experiment.

Observations on the Structure of the Stomachs of different Animals, with a View to elucidate the Process of converting animal and vegetable Substances into Chyle. By Everard Home, *Esq. F.R.S.* Read April 30, 1807. [*Phil. Trans.* 1807, *p.* 139.]

The author's observations on the stomachs of the porpoise and of ruminating animals, contained in two former communications, led him to believe that the fourth stomach of ruminating animals is subdivided during life, in a greater or less degree, into two cavities. In the camel, and in some others, this division is permanent. In the bullock, sheep, &c. it is only occasional. This arrangement leads to a presumption, that in the fourth stomach the food undergoes two changes, the one preparatory to the other.

With a view to investigate the subject, Mr. Home describes the internal structure of a series of stomachs, which he observes to form principal links in the gradation from the most perfectly ruminating to the truly carnivorous animals.

For ascertaining correctly the form of any stomach, it was found that it should not be distended at the time of the animal's death; that it should be examined as early as may be after death; and that its form is best shown by gently distending it with air.

For the purpose of examining its internal membrane, it should be inverted previous to inflation; by which means, the folds that are generally observable in that membrane wholly disappear, as they arise merely from its want of contractility, when compared to the more elastic nature of the external coats.

The stomachs of which Mr. Home has given descriptions, accompanied with drawings of most of them, are those of the turkey, cod, hare and rabbit; beaver and dormouse; the water rat, common rat and mouse; the horse and ass; the kanguroo; the hog, pecari, and elephant; the mole; the stoat and armadillo; together with those of men and of dogs. The circumstances principally noticed in these descriptions are the extent to which the articular lining is carried; the appearance of the membrane that succeeds its termination; the situation and forms of any glands that are observable in the several parts of each stomach, and, more especially, a contraction which in some animals forms a permanent division of the last stomach into two parts, and even in others, as man, where no such division has been observed. Mr. Home is of opinion, that a similar, though partial, subdivision takes place during life by muscular contraction, as some traces of it may in general be detected after death, if the stomach be examined early and under favourable circumstances.

From the anatomical structure of the different stomachs described in the present and two preceding papers, Mr. Home is led to consider the functions belonging to the several parts of that organ.

The cuticular lining of the first, second, and third stomachs of ruminants has occasioned them to be considered as mere reservoirs; but since they are supplied with secretions, he thinks that, like those in the crops of birds, they assist in softening the food and in preventing fermentation. It appears also, from Dr. Stevens's experiments, that even these have somewhat of a solvent power.

Mr. Home makes a remark that he thinks deserving of notice, respecting the preparation of food in the first cavity; namely, that a certain quantity is always retained there, even though the animal has fasted for a whole week previous to its death.

The digestive process of ruminants he considers as very similar to what takes place in birds with gizzards, who swallow the food without mastication. It is then softened in the crop; after which the gizzard, like the grinders of the ruminant, prepares it for solution and conversion into chyle.

This general resemblance having led him to a more minute comparison of their glandular structure, it was observed, that at the entrance into the gizzard there is a glandular zone that secretes the true gastric juice, having the power of dissolving the food. This *solution*, according to Mr. Home, is the second step in the process of digestion, and is effected in ruminants by the cardiac portion of the

fourth stomach. The glands, from which this fluid is secreted, are very distinctly seen in the upper portion of the stomach of the deer; and in the lower portion are other glands, which secrete a fluid, to complete the process of digestion by forming chyle.

In the porpoise and whale tribe the two processes of solution and chylification are completely separate, as there can be no doubt of the food being dissolved before it arrives at the third stomach; since the opening leading into that cavity is too small to admit anything but fluids to pass, and the analogy between the second and third cavities of the whale, with the two portions of the fourth of ruminants, is very great.

In the cod there are only two cavities, one for solution, with a structure that bears a strong resemblance to that of the second cavity of the porpoise, and having orifices similar to those in the plicated portion of the stomach of the deer. Beyond this first cavity in the cod, the food cannot pass till it is broken down; so that the analogy between the fish and the porpoise is very strong: in both one and the other, solution is a step previous to the formation of chyle, which is performed by secretions from glands of a different structure, and applied to the food in a different cavity. And in this, the bird, the fish, and the whale tribe, all agree.

The animals most nearly allied to the ruminants in their mode of digestion are those which occasionally ruminate, as the hare and the rabbit; and in these also that part of the stomach nearest to the œsophagus is never emptied, as happens in perfect ruminants.

The next variety in the process of digestion is that of the beaver and dormouse, in both of which there is a glandular structure, peculiar in quantity, which seems to correspond with the solvent glands of other animals, and renders it probable that an increased secretion of solvent liquor renders rumination unnecessary.

Next to these follow animals with a cuticular reservoir, in which the food macerates before it is submitted to the process of digestion, as in the water rat, the common rat, and the mouse. In the first there is a permanent division, but in the two last it is only muscular.

The stomachs of the horse and ass are also very nearly allied to these in their structure, and must be considered of the same kind.

That of the kanguroo is peculiar, having pouches at its cardiac extremity lined with a glandular membrane. This stomach is, from its unusual length, more capable of subdivision into a number of cavities by muscular contraction; and Mr. Home thinks this form likely to facilitate regurgitation for the purpose of ruminating, which this animal has been seen to perform.

The other stomachs that are observed to have pouches at their cardiac extremity are those of the hog, pecari, hippopotamus, and elephant. That of the hog, excepting for a single pouch at its cardiac extremity, would very much resemble those of the horse or rat.

The stomachs that come next under consideration are those adapted to digest *animal* food. In these there is little difference observable in the cardiac portion (because animal substances are easily dissolved);

but the principal difference observable is at the pylorus, because, as Mr. Home conceives, some animal substances, after solution, are less readily changed into chyle than others.

In the most perfectly carnivorous animals, the internal membrane is extremely uniform in its appearance; but even in these a division or capability of it by muscular contraction is observable.

The first instance in which Mr. Home remarked this division in the *human* stomach, was in a woman who was burnt to death, after having been unable to take much nourishment for several days before. But since that time, as he has taken frequent opportunities of examining the human stomach recently after death, he finds that this contraction may generally be met with in a greater or less degree; but when a body is examined as much as twenty-four hours after death, this appearance is rarely to be met with; which accounts for its not having been before particularly noticed.

The series of stomachs arranged according to their structure, which has been given, includes the principal peculiarities that appear to Mr. Home capable of influencing the process of digestion: it is, however, considered only as a first imperfect attempt, which he hopes that other inquirers will render more complete.

Experiments for investigating the Cause of the coloured concentric Rings, discovered by Sir Isaac Newton, *between two Object-glasses laid upon one another. By* William Herschel, *LL.D. F.R.S.* Read February 5, 1807. [*Phil. Trans.* 1807, *p.* 180.]

The account, given by Sir Isaac Newton, of these coloured arcs, appeared to Dr. Herschel highly interesting, but he was not satisfied with the explanation of them. Sir Isaac Newton accounts for the production of the rings, by ascribing to the rays of light certain fits of easy transmission and alternate reflection; but this hypothesis seemed not easily to be reconciled with the minuteness and extreme velocity of the particles of light.

With the view of inquiring further into the cause of these phenomena, Dr. Herschel, so long since as the year 1792, borrowed of this Society the two object-glasses of Huygens, one of 122, and the other of 170 feet focal length. Notwithstanding various interruptions, the series of experiments, made in the course of this time, has been carried to a considerable extent; and Dr. Herschel thinks the conclusions that may be drawn from them, sufficiently well supported to point out several modifications of light that have been totally overlooked, and others that have not been properly discriminated.

The aim of the present paper is to arrange the various modifications of light in a clear and perspicuous order; but Dr. Herschel reserves his sentiments upon the cause of the formation of concentric rings, for a subsequent communication.

The first section describes different methods of making one set of concentric rings visible. The first method consisted in placing a double convex lens, of 26 inches focus, upon a piece of glass, of which

the upper surface was plain and polished, but the under surface either plain, concave, or convex. These were placed before a window, in such a position that the light fell upon the lens at about 30° from the perpendicular, and was received by the eye, at an equal elevation, on the opposite side.

Instead of the surface of glass, a metallic surface was next substituted; and the same lens placed upon it, gave the same appearance of similar concentric rings.

Moreover, it is not necessary that the surface of the under substance should be plain. It may be either concave or convex; so also may the upper surface, in contact, be either convex or concave, provided that when a concave surface is applied to another that is convex, the radius of concavity be greater than that of the convexity to which it is applied.

The second section treats of seeing the same rings by transmission, which, of course, admits the same variety in the forms of the surfaces in contact, but will not allow either of them to be metallic.

The third section distinguishes the several images, of any object, that are reflected from the different surfaces of several plates of glass, laid one upon another, on account of the use that may be made of these images, in assisting to discern the complicated phenomena produced in succeeding sections.

In the fourth section a second series of rings is produced, by placing the lens upon a piece of looking-glass, which occasions the primary set to be seen a second time by reflection. But as this is less bright, the primary set must first be obscured, by bringing the second reflected image of a pen-knife, or other pointed body, over it. In this case there are three images of the pen-knife. The second obscures the primary set of rings; the third shows them to the greatest advantage.

The same varieties of contact which were found to make one set of rings, may, of course, be applied to make a secondary set, if there be a reflection beneath sufficiently bright to render it visible.

The fifth section treats of three sets of rings, produced by increasing the number of reflecting surfaces, as when a slip of glass is interposed between the lens and the looking-glass of a former experiment, or when the lens, laid upon two slips of glass, is placed on a plain metallic reflector.

The sixth section pursues the same complicated appearance, as far as four sets of rings, and shows how they may be discerned, by means of the reflected images of the pen-knife.

In the seventh section the size of rings is considered, so far as it depends on the curvature of the surfaces; but Sir Isaac Newton having already treated this part of the subject at large, Dr. Herschel does not think it necessary to enter further into it.

In the eighth section, the species of contact requisite for exhibiting the rings is mentioned, the size of them being considerably affected by pressure. They grow larger when the two surfaces that form them are pressed closer together, and diminish in proportion as the

pressure is removed. The smallest ring of a set may thus be increased to double or triple its former diameter. But to produce that which may properly be called contact, mere pressure is not sufficient; and it will be necessary to give a little motion laterally backwards and forwards, accompanied with moderate pressure.

The number of the rings, which may be seen at once, varies from eight or ten to as many as twenty, accordingly as the light is less or more favourable. As the size of the rings is altered, so the colours of them are much affected by pressure. When a convex surface, of fifteen feet radius, is laid upon a plain surface, if the colour which first appears be red, a moderate pressure will convert it into a ring of red, with a green centre; and in the same manner, by increase of pressure, the green will give place to red; and so alternately for six or seven times, till at last, *in absolute contact*, the centre becomes black, surrounded by white.

The twelfth section describes the successive development of all the prismatic colours, by using lenses of greater radii. For though a small lens, of two inches, shows nothing but black and white in the series of rings that surround the centre of absolute contact, with a lens of four inches a faint red colour begins to appear in the outward rings; and this redness will be more manifest with radii of five, six, and seven inches; but the rings will not assume a green colour till a lens is used of sixteen, eighteen, or twenty inches: but it must be observed, that this and other colours appear soonest when the lens is not kept in such contact as to give a black centre.

With a lens of twenty-six inches, violet, indigo, or blue, may first be discerned at the centre. With one of thirty-four, the white surrounding the black inclines to yellow; with forty-two or forty-eight, yellow *rings* become visible; with fifty-nine, blue rings are plainly visible; with ten feet, orange may be distinguished from yellow, and indigo from blue; with fourteen feet, violet becomes visible.

When the Huygenian lens, of 122 feet, is well settled, the central spot, which in small lenses appeared black, is diluted, and drawn out into violet, indigo, and blue, surrounded with an admixture of green; while the white ring that surrounded the black spot is also subdivided, and blending with the green edge, surrounds it with yellow, orange, and red.

The order of the colours, whether the rings are seen by reflection or transmission, is such, that the most refrangible of each ring are toward the centre; but the black of one set corresponds in position to the white of the other, and the red to the green, so that the dimensions of rings, of the same colour, in each are not alike.

Hence a sudden change of colours may be produced, in each set, by intercepting that light by which they were before seen, and occasioning them to be seen by the opposite; and this alteration of colour is accompanied with an immediate change of size.

In several of the succeeding sections Dr. Herschel explains, by reference to figures, the courses of the rays by which each appearance is seen, and refers them each to the surface from which they are reflected.

He next examines which are the reflecting surfaces, by means of certain scratches, and other defects.

In the 25th and 26th section he finds, by means of similar defects, that the surfaces in contact are alone concerned in the formation of rings; and in the 24th and 28th section he discovers, by various irregular surfaces which he employs, as 1st and 4th surfaces of two glasses in contact, that these are not concerned in the production of rings. And in the 27th section he observes, that the colour of the under glass does not affect the primary set of rings.

The results of the foregoing experiments are,—

I. That only two of the surfaces are essential to the formation of concentric rings.

II. That these two must be of a certain regular construction, so as to form a central contact.

III. That rays, from one side or the other, must pass through one of the surfaces at or near the point of contact to the other surface, and be reflected from it.

And IV. That in all these cases a set of rings will be formed, having their common centre in the point of contact.

The cause of these phenomena, Dr. Herschel says, must be either in the nature of the rays themselves, or in the surfaces; and if it can be shown that the disposition to fits of easy transmission and reflection does not exist, a proposition of accounting for them by modifications occasioned by the surfaces, he thinks, will find a ready admittance.

In section 30, he shows that the word transmission will not apply to the case where rings are produced by placing a lens upon a metallic surface, and wishes to substitute the word absorption.

In section 31, Dr. Herschel contends that a plate of air, of the thinness which is supposed sufficient, will not give coloured rings, because in a case of circumferential contact, where a concave surface was applied to one that was convex, of very little larger radius, he could not perceive any appearance of colour.

In section 32, he places a piece of plain glass, four tenths of an inch square, on a concave glass mirror of 10 feet focus, but could observe no rings or colours.

In section 33, he does not find that a secondary set of colours, produced in the usual way, is altered by being seen through a wedge of air, occasioned by the interposition of card between the edges of two slips of glass.

And finally, in section 34, Dr. Herschel could discern no colours when two slips of plain glass, two inches long, were in contact at one extremity, and distant only $\frac{1}{1000}$th of an inch at their other extremities; although in the first half-inch from their contact, the several distances which Sir Isaac Newton considers as capable of producing ten successions of colours, must have occurred.

Dr. Herschel therefore infers, that the rays of light have no disposition to be alternately reflected and transmitted at certain intervals of space; but the examination of the various modifications that light

receives by its approach to, entrance into, or passage by differently disposed surfaces, he reserves for a second part of this paper, to be hereafter communicated.

On the Economy of Bees. In a Letter from Thomas Andrew Knight, *Esq. F.R.S. to the Right Honourable Sir* Joseph Banks, *Bart. K.B. P.R.S.* Read May 14, 1807. [*Phil. Trans.* 1807, *p.* 234.]

During the progress of the various experiments on vegetation, of which Mr. Knight has communicated accounts to the Society, he has had opportunities of paying considerable attention to the economy of bees, and has observed many interesting circumstances, that appear to have been overlooked by former writers.

A general opinion prevails that every hive remains at all times unconnected with other colonies in the neighbourhood, and that strangers are always considered as enemies. Mr. Knight, on the contrary, has in several instances witnessed a friendly intercourse to take place between different colonies, and he imagines it to be productive of important consequences in their political economy.

Having observed several bees flying one evening at a later hour than they usually work, he endeavoured to discover how they were employed, and he found them to be passing in a direct line from one of his own hives to that of a cottager, about 100 yards distant. There was a considerable degree of bustle and agitation in each of these hives; every bee as it arrived seemed to be stopped and questioned at the mouth of each hive, but there was no appearance of hostility or resistance. This kind of intercourse continued, in a greater or less degree, during the eight following days, and appeared to be amicable for the whole of that time. But on the 10th their friendship terminated in a quarrel, and they fought desperately.

Mr. Knight has had other opportunities of observing a similar intercourse with the same result; but he has reason to think that it not unfrequently terminates in a junction of the two swarms; and he remembers to have observed, many years ago, circumstances perfectly similar in one hive followed by desertion of the labouring bees, who left the drones alone in possession of the hive, but without anything to live upon. He further thinks, that when a junction is determined upon, they remove immediately, and return only during the day for the purpose of carrying off the honey.

Mr. Knight has also remarked the manner in which colonies of bees, proposing to emigrate, fix upon their future habitation. He has frequently noticed an examination of certain hollow trees to take place for many days together by detachments of bees, from twenty to fifty in number. This examination was not confined to the mere cavity, but extended to the external parts of the tree above; as if they were apprehensive of injury from moisture by any perforation.

Their scouts must apparently have some means of communicating information of their success, without which it cannot be supposed that others would accidentally meet at a mile distance from their

hive. The search is, in general, continued for about a fortnight previous to their removal.

Mr. Knight has observed, that in this case also colonies will sometimes unite; for he has seen, in two instances, a swarm received into a cavity, of which another swarm had previous possession, without opposition. He infers, therefore, that some preceding intercourse must have taken place between the two swarms, although anything like an agreement between them be scarcely consistent with the limits generally supposed to be set to the instinctive powers of the brute creation.

When a young swarm issues from the parent hive, they generally soon settle on some neighbouring bush or tree, wholly unprotected from rain or cold; and their object apparently is merely to collect their numbers previous to removal to the place they have fixed upon for their future residence. Their readiness to accept a hive as a substitute, may appear to militate against any supposed predetermination; but Mr. Knight is disposed to consider this as an hereditary habit produced by domestication, and confirmed in the breed by the uniform practice of many succeeding generations as a secondary instinct. Accordingly, the original native propensity to migrate, remains more strong in some families of bees than in others.

Similar hereditary propensities are observable in the offspring of many other domesticated animals. In the dog, more especially, appear the passions and propensities of its parent. A young spaniel, brought up with terriers, showed no marks of emotion at the smell of a polecat, which instantly irritated the young terriers; but it pursued a woodcock with clamour and exultation at first sight; and the young pointer stands trembling with anxiety, with his eyes fixed and his muscles rigid, the very first time that he is conducted into the midst of a covey of partridges.

These peculiarities of character can be considered as nothing but hereditary propensities or acquired instincts; and are modifications, capable of endless variation, in adapting animals to different countries and different states of domestication.

Mr. Knight's further observations relate to the bee-bread and the bees' wax. Respecting the former, he agrees fully with Mr. Hunter, that the substance generally collected on the thighs of bees is the farina of plants for feeding their young; but he observes that they occasionally carry other substances, and for other purposes, in the same manner. With regard to the wax, he is not of Mr. Hunter's opinion, that it is a secretion exuding from between the scales of the abdomen, but thinks that it is of vegetable origin, collected by the bees, and deposited between the scales for facility of conveyance, and for giving the requisite temperature for being moulded into combs.

Observations and Measurements of the Planet Vesta. *By* John Jerome Schroeter, *F.R.S.* Read May 28, 1807. [*Phil. Trans.* 1807, *p.* 245.]

The observations contained in Mr. Schroeter's communication, comprise those of Dr. Olbers, made at Bremen, from the 29th of March to the 6th of May, and those of Mr. Bessel at Lilienthal, from the 1st of April to the 11th of May; from which it appears that this planet, now called Vesta, became stationary between the 8th and 11th of May, and is now progressive.

Mr. Schroeter endeavoured also to ascertain her magnitude; with magnifying powers of 150 and 300 applied to a 15-feet reflector she seemed equal to a star of the 6th magnitude, but without any appearance of a disc. Mr. Schroeter, and his assistant, both saw the planet at that time with the naked eye.

As they had formerly observed Ceres, Pallas, and Juno, with a 13-feet reflector, and with eye-glasses magnifying 136 and 288 times, they now examined Vesta with the same telescope and the same powers, and found her appearance to be exactly the same, her apparent diameter not exceeding $\frac{488}{1000}$ths of a second, which Mr. Schroeter says is only one half the apparent diameter of the 4th satellite of Saturn. Mr. Schroeter considers the intensity and unsteadiness of its light, together with its extraordinary smallness, as very remarkable for a body which, according to the calculations of Dr. Gauss, is in the same region between Mars and Jupiter, in which the three other lately discovered planets perform their revolutions round the sun.

A new Eudiometer, accompanied with Experiments, elucidating its Application. *By* William Hasledine Pepys, *Esq.* *Communicated by* Charles Hatchett, *Esq. F.R.S.* Read June 4, 1807. [*Phil. Trans.* 1807, *p.* 247.]

After some preliminary observations upon the important part that atmospheric air performs in numerous processes of nature and art, and upon the variety of other gaseous bodies now known, Mr. Pepys traces cursorily the progress of eudiometry from Hales, who first observed a contraction upon the admixture of atmospheric air with an air that he had obtained from spirit of nitre and pyrites. The cause of this contraction, and the nature of the nitrous gas that occasioned it, were more distinctly discovered by Dr. Priestley, who also pointed out the use to which it might be applied for ascertaining the purity of air; and he employed for that purpose a graduated tube, which he denominated an eudiometer.

Phosphorus, and the liquid sulphurets, were afterwards substituted for nitrous gas; but these being found tardy in their operation, or if accelerated by heat fallacious in their results, Mr. Davy proposed the solutions of sulphate, or muriate of iron impregnated with nitrous gas, as sufficiently sudden in their action, and more uniformly free from contamination by other gases.

Much, however, in Mr. Pepys's estimation, remained to be done in the mechanical part of the apparatus, and in the course of various experiments on these subjects, that it might be rendered more commodious in its application, and capable of giving correct results with the utmost minuteness.

The instruments which he proposes consist of two tubes, one larger and one smaller, with a bottle of elastic gum to each, and a glass cylinder, or cistern, of the same length as the tubes. The larger of the tubes, containing one cubic inch, is the principal measure, and is divided into hundredths; the smaller is intended to measure fractional parts; and in this each of the former divisions is divided into ten parts, or thousandths of the cubic inch.

One of the bottles of elastic gum has attached to it a tube, which serves as a perforated stopper to the larger measure, through which any liquid to be applied to the gas under examination may be forcibly injected. When the full contraction has taken place, the measure is immersed in the cistern, filled with mercury or with water, according to circumstances, and the contraction noted. But if the surface is found not to correspond with a division of the principal measure, the smaller tube, which slides through a cork in the bottom of the cistern, is to be passed up till its extremity (which is open) reaches the gas contained, and the fractional part is then withdrawn by means of its elastic bottle, and measured with the greatest precision.

Mr. Pepys next proceeds to an account of various gases, of which he tried the purity by means of this apparatus, as nitrous gas, containing $\frac{18}{100}$ of impurity, oxygen gas $\frac{1}{100}$; atmospheric air, of which $\frac{21}{100}$ were absorbed; carbonic gas, which left $\frac{1}{100}$, and sulphuretted hydrogen $\frac{6}{1000}$ of impurity.

In the analysis of compound gases also, he imagines it will be found peculiarly useful. He recommends using the solutions hot, which facilitates chemical union, but prevents the absorption of carbonic and other gases by the mere water of solution, which might otherwise take place.

In order to avoid the expansion that would be occasioned by handling the measure, Mr. Pepys employs a pair of forceps, having circular extremities lined with cloth, for grasping the tube.

The elastic gum, he observes, is so little acted upon by chemical agents, that a great variety of them may be employed, and may with facility be used at any temperature.

Observations on the Nature of the new celestial Body discovered by Dr. Olbers, *and of the Comet which was expected to appear last January in its return from the Sun. By* William Herschel, *LL.D. F.R.S.* Read June 4, 1807. [*Phil. Trans.* 1807, *p.* 260.]

It was on the 24th of April that Dr. Herschel first saw the new planet Vesta; but though he saw her again on the 25th, he could not determine which of several stars he noted was the planet, for

want of sufficient motion at that time. He examined her at that time with a power of 460, but could not distinguish anything in her appearance different from surrounding stars of equal brightness.

On the 21st of May, when he had learned her position more precisely from the Astronomer Royal, he observed her again with the same power, but could discern no appearance of a planetary disc.

On the 22nd he perceived her to have moved since the preceding evening, and now examined her with increased powers of 460, 577, and 636, but could find no difference between the planet and a fixed star, the 463rd of Bode's Catalogue.

Since Vesta thus bears to be examined by high magnifying powers without apparent enlargement of her disc, which is the test by which Dr. Herschel formerly determined the apparent discs of Ceres, Pallas, and Juno, to be spurious, he considers her as belonging to that formerly unknown species of celestial bodies which he has termed asteroids.

May 24th.—Dr. Herschel compared Vesta with the Gregorian planet, and found that with a power of 577 her apparent disc was about one ninth or one tenth as large; and with his 20-feet reflector, of 18¾ inches aperture, she had no surrounding atmosphere or nebulosity.

Dr. Herschel's observations on the comet, which follow, were confined to three days, January 27, January 31, and February 1. It was near the electrometer of the constellation, called by Bode *Machina electrica*. Its coma was of an irregular round form, extending six or seven minutes in diameter.

Upon revising his observations of sixteen telescopic comets, Dr. Herschel remarks, that fourteen have been without any visible solid bouy in their centre, and that the other two had but an ill-defined small central light, which did not deserve the name of a disc.

On the Quantity of Carbon in carbonic Acid, and on the Nature of the Diamond. By William Allen, *Esq. F.L.S. and* William Hasledine Pepys, *Esq. Communicated by* Humphry Davy, *Esq. Sec. R.S. M.R.I.A.* Read June 18, 1807. [*Phil. Trans.* 1807, *p.* 267.]

The experiments, which form the subject of the present communication were undertaken, not only on account of the difference between the estimates that have been made of the quantity of carbon in carbonic acid, but because those of Guyton de Morveau, which are most frequently preferred at this time in various systems of chemistry, appeared liable to many objections, from the manner in which they were conducted; while the original experiments of Lavoisier, on the contrary, appear to have been performed with much accuracy, and had moreover been confirmed by Mr. Tennant in his researches on the nature of the diamond.

The design of the authors was to consume certain known quantities of diamond and of other carbonaceous substances in oxygen gas; for which purpose it had been originally their intention to employ

the sun's rays, by means of a powerful lens; but, considering the uncertainty of a favourable opportunity in this country, they resolved to employ an apparatus consisting of two mercurial gas-holders, with a tube of platina interposed between them in a horizontal position, and passing through a small furnace, by which the tube and its contents might be heated to any degree requisite for the combustion of the substance employed.

Into this tube the diamond or other variety of carbonaceous matter was introduced in a small tray, also of platina; after which, by opening a due communication with each gas-holder, the oxygen was made to pass freely over the surface, from one gasometer to the other, during the continuance of the heat, and subsequently examined by means of the eudiometer lately described by Mr. Pepys.

Having found that oxygen gas was liable, notwithstanding every precaution, to be deteriorated by keeping, the authors were careful to prepare it, on all occasions, within an hour or two of the time of using it, from the hyperoxygenized muriate of potash. Its purity was also ascertained before every experiment. The solution employed for this purpose was the solution of green sulphate of iron, saturated with nitrous gas; and lest any increase might have been occasioned by the extrication of this gas from the solution, the simple sulphate alone was subsequently employed, so that the residuum enabled them to determine exactly the quantity of oxygen contained in the gas.

Their charcoal was prepared from different kinds of wood, sawed into slips, and gradually heated in small crucibles covered with sand, and ultimately retained in a white heat for forty minutes. By this treatment

Fir yielded	18·17	per cent.
Lignum Vitæ	17·25	——
Box	20·25	——
Beech	15	——
Oak	17·40	——
Mahogany	15·75	——

Having next examined the absorbent power of charcoal, and found that the weight which it gains by exposure to air is principally attributable to water, the charcoal to be employed in any experiment was always subjected to a red heat immediately before using it, and weighed as expeditiously as was consistent with accuracy.

Since the volumes of gas employed and produced would be influenced by temperature, as well as barometric pressure, the states of both barometer and thermometer were noted at the time of every experiment, and allowance was made by adding, or subtracting, $\frac{1}{480}$th part of every degree below or above 60°.

The exact weights of certain measures of oxygen gas, and of carbonic acid gas, were also carefully examined, by allowing a glass globe, previously exhausted and weighed, to receive a given measure of either of these gases from a gasometer. By the increase of weight acquired in each case, it was found that 100 inches of oxygen gas

weighed 33·82 grains, and that the same measure of carbonic acid gas weighed 47·26 grains.

In the first experiment, which they made upon the combustion of charcoal, four grains of box-wood charcoal were employed, and it was found that only $\frac{1}{50}$th of a grain remained of a white ash. The volume of gas employed appeared unaltered after the heat had subsided; but the combustion of 3·98 grains of charcoal had produced 29·13 inches of carbonic acid gas, or 13·76 grains; so that according to the experiment, 100 grains carbonic acid gas contains 28·92 charcoal. But by a computation founded on the quantity of oxygen consumed, it would appear that the quantity of charcoal is only 28·77 per cent.

In their next experiment 2·49 grains of diamond, in small fragments, were consumed. In this case, as in the former, the combustion caused no apparent increase or diminution of the quantity of gas; but there were found to be 18·20 inches of carbonic acid gas, in which the diamond would appear to be contained in the proportion of 28·95 per cent. But again, by estimation from the weight of 18·20 inches of oxygen consumed, the proportion of diamond was slightly different, being 28·81 per cent.

In this experiment, the authors could perceive no appearance of moisture or dullness on the surface of the quicksilver, or sides of the glasses; and they observed that the diamond had left no discolouration of the tray, and no residual ash.

In a second experiment eleven small diamonds, weighing 4·01 grains, were consumed; and they produced 13·91 grains of carbonic acid gas, in the proportion of 28·82 parts diamond for each 100 of gas.

By a similar experiment upon stone-coal from Wales, such as is employed by maltsters, the quantity of carbonic acid gas produced was found to contain 28·20 per cent. of coal; but in this case the proportion deduced from the quantity of oxygen consumed, rather exceeded the estimate formed from the carbonic acid, instead of being less, as in former experiments.

Carbonic acid formed in the same manner from the combustion of plumbago, contained 28·46 of carbonaceous matter, whether estimated from the gas produced, or from the oxygen gas consumed.

From the average result of these five experiments, the authors conclude that 100 parts of carbonic acid contain 28·6 of carbon, a quantity rather greater than appeared to Mr. Tennant, who did not find it more than 27·8; but this difference may easily be accounted for, by the different modes of operating.

The authors conclude, that the estimate given by Lavoisier at 28, which is between that of Mr. Tennant and their own, is very near the truth.

2ndly. That the diamond is pure carbon.

3rdly. That well burned charcoal contains no hydrogen, but soon absorbs moisture from the air, which would occasion fallacious results.

4thly. That charcoal is not an oxide of carbon; since, when rightly prepared, it requires quite as much oxygen for its combustion as diamond.

And lastly, That the diamond differs from charcoal, solely in the firmness of its aggregation, which is generally known to be an obstacle to every chemical change.

An Account of the Relistian Tin Mine. By Mr. Joseph Carne, *in a Letter to* Davies Giddy, *Esq. M.P. F.R.S.* Read May 7, 1807. [*Phil. Trans.* 1807, *p.* 293.]

Mr. Davies Giddy communicated a letter from Mr. Joseph Carne, giving an account of the Relistian Mine in Cornwall, in which rounded pebbles were found at the depth of seventy-five fathoms from the surface. The description is accompanied with a section of the mine, and plan of the lode.

The lode has been seen at various depths, from twelve to ninety fathoms, but its width then diminishes rapidly toward the east, but more gradually toward the west. The substances not metallic are schist, chlorite, and quartz. There is an engine-shaft at eight fathoms from the lode, on the north side from which levels are driven to the lode in the direction of a flucan, or cross lode, which cut the main lode nearly at 45° from N.W. to S.E. The distance, in this oblique direction, is about fifteen fathoms; for the first ten fathoms there was only one flucan, of four inches width. Then it became divided into four parts, so much divergent from each other, that at the depth of seventy-five fathoms the extreme branches extended twelve feet in width; where the flucan reached the lode at this depth, there was first discovered a little copper, and then a body of pebbles, the section of which was about twelve feet square. In this part of the lode the schist greatly predominates; of course (says Mr. Carne) the pebbles are schistose, cemented in some parts by chlorite, in others by oxide of tin, which is generally crystallized, and in some of the crevices there is a little copper pyrites. The pebbles did not continue in a body to the height of more than two fathoms; but scattered bunches were found four fathoms above, and six fathoms below the place where they were first discovered.

At the depth of sixty-five fathoms, and in the lode adjacent to the flucan, on each side, had been found also a great number of schistose stones; but these were angular fragments, not rounded; nor was there any tin in or about them.

An Analysis of the Waters of the Dead Sea and the River Jordan. By Alexander Marcet, *M.D. one of the Physicians to Guy's Hospital. Communicated by* Smithson Tennant, *Esq. F.R.S.* Read June 18, 1807. [*Phil. Trans.* 1807, *p.* 296.]

This analysis is preceded by a short abstract of the notice taken of the Dead Sea by various ancient authors, by Strabo, by Tacitus,

and by Pliny, as well as by Pococke, Volney, and other modern travellers, who all concur with respect to the intense saltness of the water, which is such as to prevent either animals or vegetables from living in it,—a peculiarity from which it has derived its name.

The only analysis which Dr. Marcet has been able to find recorded, is that of Macquer, Lavoisier, and Sage, in the *Mém. de l'Académie* for 1778. But these chemists had not attained that accuracy of which modern analysis is susceptible, and appear not to have bestowed upon the subject that attention which is requisite in minute analytical experiments.

The quantity of the water of the Dead Sea, which was the subject of Dr. Marcet's experiments, amounted only to about 1½ ounce; but it was in a phial, carefully corked, and appeared to be in a state of perfect preservation. The smallness of the quantity, and difficulty of obtaining a further supply, occasioned Dr. Marcet to be anxious that none might be wasted in previous trials. He accordingly began by a variety of comparative experiments on artificial solutions, in order to ascertain the accuracy of various modes of operating; and as he knew by the analysis of Lavoisier and his associates, that the principal ingredients which he might expect to find were the muriates of soda, of magnesia, and of lime; and as the general effect of reagents tended to confirm their results, excepting that they indicated the presence of a small quantity of selenite that had been overlooked, the solutions on which Dr. Marcet's experiments were conducted were made to consist of the same ingredients, and in proportions which he previously ascertained with great precision.

The first step, which appeared indispensable, was to determine the proportions of acid and base in each of the three muriates.

The composition of muriate of lime was ascertained by pouring a quantity of muriatic acid on a piece of marble of known weight, and larger than was necessary to saturate the acid: after saturation and evaporation of the solution, the residuum was heated to redness, and carefully weighed. From the weight of this residuum, after subtracting the quantity of lime contained in a given weight of marble, which, by a number of experiments performed jointly with Mr. Tennant, was found to be 56·1 per cent., it appeared that 100 parts of muriate of lime consist of 50·77 lime, and 49·23 muriatic acid.

To discover the proportional parts of muriate of magnesia required a synthetic process somewhat different. To a known weight of magnesia previously calcined, was added such a quantity of muriatic acid as would dissolve a known weight of marble; and after the whole of the magnesia had been dissolved, the excess of acid was saturated with marble, by which the redundant quantity was ascertained. Hence the quantity of acid combined with the given weight of magnesia was deduced, and the component parts of muriate of magnesia determined to be 43·99 magnesia, and 56·01 acid.

The proportion of acid and alkali in muriate of soda, was found, by various methods, to be 46 acid, and 54 soda. The best method appeared to be that of precipitation by nitrate of silver; the quan-

tity of acid in luna cornea having been previously determined to be 19·05 per cent, by means of the weight of luna cornea, produced by a given quantity of acid.

The method found most successful for analysing the artificial compound solution, consisted in dividing it into two portions, from one of which the acid was precipitated by nitrate of silver, and its weight ascertained. From the other the lime was precipitated by oxalate of ammonia, and the magnesia by caustic potash. The respective quantities of acid, combined with each, being thence deduced by assistance of the preceding experiments, the quantity of muriate of soda could be inferred from the remaining quantity of acid.

By this method a compound solution known to contain

8·17 muriate of lime	appeared by trial to contain	8.14
26·10 muriate of magnesia		25·62
25 muriate of soda		25·47

After this series of preliminary experiments, Dr. Marcet proceeded to the analysis of the water itself, of which 100 parts by evaporation, at a temperature of 180°, left a residuum of 41, and at 212°, 38½.

When 100 grains of the water were treated by muriate of barytes, they gave a precipitate of one-tenth of a grain of sulphate of barytes.

Another portion, weighing 250 grains, by addition of nitrate of silver yielded a precipitate of 163·2, a quantity equivalent to 31·09 acid. Oxalate of ammonia being then added, occasioned a precipitate containing 4·814 pure lime; and hence the quantity of muriate of lime is computed to be 9·48.

The clear solution, containing the nitrates of magnesia and of soda, with oxalate of ammonia, having then been concentrated by evaporation, subcarbonate of ammonia occasioned a precipitate containing 11·1 of pure magnesia, which are equivalent to 25·25 muriate of magnesia.

After deducting the quantities of acid contained in these muriates from the quantity estimated by luna cornea, there remained 12·28 for acid in the muriate of soda, which, by estimation from preceding experiments, was thus found to be 26·69.

These results being brought into one view, the water of the Dead Sea appears to contain, in every 100 parts,

Muriate of lime	3·792
Muriate of magnesia	10·100
Muriate of soda	10·676
Sulphate of lime	0·054
	24·632

being the total amount of saline matter when perfectly dry.

The same chemical reagents as were used to discover the general properties of the water of the Dead Sea being applied to that of the river Jordan, were found to produce analogous effects. But the quantity of saline matter was so small, that on evaporation of 500 grains at about 200°, the residuum weighed only eight tenths of a grain.

The inference drawn by Dr. Marcet from the general resemblance is, that the river Jordan might possibly be the source of the saline ingredients of the Dead Sea, or that the same source of impregnation might be common to both.

The Bakerian *Lecture, on some new Phenomena of chemical Changes produced by Electricity, particularly the Decomposition of the fixed Alkalies, and the Exhibition of the new substances which constitute their bases; and on the general Nature of alkaline Bodies. By* Humphry Davy, *Esq. Sec. R.S. M.R.I.A.* Read November 19, 1807. [*Phil. Trans.* 1808, *p.* 1.]

In this lecture Mr. Davy, after recalling to our recollection the series of experiments described in his last Bakerian lecture, in which various bodies, consisting of known ingredients, having the highest known affinities for each other, were decomposed by the agency of electricity, reminds us also of a conjecture which he then formed, that a greater intensity of the same power might likewise overcome the affinities of other elements which had not hitherto been separated, and proceeds to inform us that this conjecture is now fully verified; for that by a laborious experimental application of the powers of electro-chemical analysis, he has been enabled to decompose various bodies which have appeared simple when examined by common chemical means.

Those of his experiments which are in the most mature state, relate to the decomposition of the fixed alkalies, and to the evolution of new and extraordinary bodies which constitute their bases.

His first attempts to effect the decomposition of these alkalies were defeated by the presence of water, since on that occasion he employed saturated solutions of them in water.

In his second attempt, since solid dry potash is a perfect non-conductor, he kept it in a state of fusion by intense heat, during electrization by a voltaic battery, consisting of 100 6-inch plates, highly charged. Under these circumstances the potash became a conductor; and brilliant light, with an appearance of flame at the negative wire, seemed to prove the development of combustible matter at this point of contact; but on account of the heat employed, no such product could be collected.

The method which he next pursued was more successful, as he found that a very slight addition of moisture renders potash a conductor, and that in this state it readily fuses by the electric power, which at the same time effects its decomposition.

When a piece of potash, weighing as much as forty or fifty grains, was exposed under these circumstances to the action of the battery, consisting of 100 plates of six inches, with 150 of four inches in intense activity, the potash began to fuse at both its points of electrization. There was a violent effervescence at the upper, or positive surface (by extrication of oxygen gas, as afterwards appeared), while at the lower, or negative surface, there was no liberation of elastic

fluid, but a formation of small globules, having a bright metallic splendour and a perfect resemblance to quicksilver; but they tarnished rapidly at their surface, and were in general soon covered by a white film, but occasionally burned with explosion and a white flame.

These globules were soon proved, by numerous experiments, to be a peculiar inflammable principle, the basis of potash.

The platina used, as the medium, for communicating the electric power, was proved not to have contributed otherwise to the result than as a conductor; for by substituting other conductors, as copper, silver, gold, plumbago, or charcoal, the same substance was formed by means of them. Its formation was also independent of the presence of air, as it took place equally under an exhausted receiver.

Soda, when acted upon in the same manner, and under similar circumstances, exhibited analogous results; but it required far greater intensity of action, and could not be decomposed in pieces which exceeded fifteen or twenty grains in weight.

That the substances thus produced were evolved by the decomposition of the alkalies, was proved by careful examination of the gas extricated from the upper surface, which was found to be pure oxygen; and again proved synthetically, by ascertaining that they absorbed oxygen gas, to which they were exposed, and that the compounds resulting were, in fact, the respective alkalies that had been employed for their formation.

The affinity of the base of potash for oxygen is such, that its metallic lustre very rapidly tarnishes in the atmosphere, by the formation of a white crust, which, by attracting moisture, is soon converted into a saturated solution of potash.

With the base of soda the phenomena are perfectly analogous, but less rapid; and when either of them is heated in oxygen gas, a rapid combustion, with brilliant white flame, is produced. Oxygen gas is absorbed in this process, and nothing emitted to affect the purity of the residual air.

It appears, then, says Mr. Davy, that in these facts there is the same evidence for the decomposition of potash and soda into oxygen and their peculiar bases, and for their recomposition, that there is for the decomposition and formation of any metallic oxide, or of the sulphuric or phosphoric acids.

In the analytic experiments no substances are present but the alkalies and a minute portion of water, which, in its common form, seems in no other way essential to the result, than by rendering them capable of becoming fluid, and conductors at a lower temperature than that at which their combustible bases burn; for if the quantity of water be increased, no such result is obtained.

These bases seem to be repelled, as other combustible substances, by positively electrified surfaces, and attracted by negative surfaces, and hence are formed at that side in the electric circuit.

The properties of these bodies are such, that in the first place it is extremely difficult to preserve them, and to confine them for experiments; such is their affinity for oxygen, that they act on almost

every fluid in which they are attempted to be kept, and more or less on the solid ingredients of the vessels in which they are examined.

The fluid substance containing the least oxygen, and consequently best adapted for preserving them, is naphtha recently distilled. In this they may be kept many days without any considerable change; and when taken out of it, their physical properties may be conveniently examined in the atmosphere while their surface is yet moist, and protected by a thin film of naphtha.

The base of potash, at the temperature of 60°, appears as a fluid globule, having the full metallic lustre, opacity, and so much the general appearance of a globule of mercury, that a difference cannot be detected by the eye. Its fluidity, however, even at this temperature, is not perfect, and at 50° it becomes a soft solid; at 40° it is malleable, and has the lustre of polished silver: at about the freezing point of water it is brittle, and when broken, its fragments exhibit a crystalline texture.

When its temperature is, on the contrary, raised to 100°, its fluidity is perfect; and when it is heated to a degree approaching to redness, it boils, and after distillation is found in its condensed form unaltered. Like metals, it is a perfect conductor of electricity; but when the globule exposed is too small for the quantity of electricity to be transmitted, it is completely dissipated with explosion, accompanied with a vivid white light.

It is also, like most known metals, an excellent conductor of heat, when brought into contact with mercury, &c. When the base of potash is brought into contact with mercury, it instantly amalgamates. If the globule of mercury be much the larger, the compound appears to be more fluid than mercury; but when the mercury exceeds in size only in the proportion of 2 to 1, the compound, though fluid at first from the heat of union, consolidates as it cools, and appears as a soft metal, similar to silver.

The fluid compound unites with all metals that have been exposed to it, acting readily even upon iron and platina.

The base of potash will also combine with other metals without the assistance of mercury, if applied to them in its fluid state; and it is remarkable, that in most instances the point of fusion of the compound is much higher than could be expected from that of the ingredients.

With sulphur and with phosphorus it also combines, and the compounds agree in appearance with the metallic sulphurets and phosphorets.

But though it resembles the metals in all these properties, it differs from all most remarkably in specific gravity, since it rises to the surface even in doubly-distilled naphtha, of which the specific gravity is only $\frac{77}{100}$th of water.

As the quantity which can be obtained is necessarily small, it is difficult to determine any points in which quantities are concerned, with minute precision; and accordingly the average obtained from several experiments, conducted with great care, and with a very

delicate balance, can only be considered as a near approximation to the truth. By an average of four such trials of its specific gravity, its weight when compared to that of an equal globule of mercury, was as 10 to 223, which gives its proportion to that of water nearly as 6 to 10, so that it is the lightest fluid body yet known to chemists.

In consequence of the great affinity of this body for oxygen, it decomposes water instantly, with great heat, accompanied with explosion and a brilliant flame. By its union with oxygen, potash is formed and is found dissolved in the water; while hydrogen, in its nascent state, uniting to another portion, becomes spontaneously inflammable at the time of its formation.

The heat during combustion is such, that when a globule of the base of potash was placed upon ice, it burned instantly with a bright flame, and a deep hole was made in the ice, which contained a strong solution of potash.

Such is the attraction of this substance for oxygen, and so great the energy of its action upon water, that it discovers and decomposes the small quantities of water contained in alcohol and ether, even when they are most carefully purified.

The properties of the base of soda are in all respects so analogous to those of the base of potash, that in this abstract it will be sufficient to notice only those circumstances in which they principally differ.

The specific gravity of the base of soda exceeds that of the base of potash; but it is inferior to that of water in the proportion of 9 to 10.

Its point of fusion is also so much higher, that at all common temperatures of the atmosphere it appears as a soft and extremely malleable metal; and it does not fuse till exposed to a heat exceeding 150° Fahrenheit, and does not become volatile in a heat sufficient to melt plate-glass.

In proportion to the less fusibility of this body, its oxidation is less rapid, and its combustion when heated less vivid than the former. In the decomposition of water also, which it effects immediately, there is no luminous appearance, although it is accompanied with violent effervescence and a loud hissing noise.

In endeavouring to determine the proportion in which each of these bases unites with oxygen to form potash or soda, Mr. Davy is again necessarily limited in accuracy by want of sufficient *quantity* of materials for experiment; but by a comparison of different methods, in each of which the results of several trials were taken, he had reason to be convinced, from their coincidence, that there could be no considerable error.

In his attempt to ascertain this point by direct union, he had various difficulties to overcome from the action of the simple base or of the compound product on the vessel employed, and from the affinity of the alkali for water, which would affect the apparent result. By an average, however, of six experiments, selected as the most carefully made, and under the most favourable circumstances, he inferred that 100 parts of potash consist of 86 base and 14 oxygen;

and in the same manner, that 100 parts of soda contain 81 base and 19 oxygen.

The other method consisted in collecting the hydrogen evolved from water by a known quantity of each base, and estimating from them the quantity of oxygen absorbed. The mean of two such experiments on the base of potash, gave the proportion of 15½ per cent. oxygen, and the mean of three trials upon soda, showed it to contain 20 per cent. oxygen.

So that the general average of both methods authorizes us to consider potash as containing 15 of oxygen, and soda 20 of oxygen, per cent.

Since oxygen had thus been proved to enter into the constitution of the fixed alkalies, Mr. Davy was led to conjecture that it might also form a constituent part of ammonia. For though the apparent conversion of ammonia into mere nitrogen and hydrogen in the experiments of Scheele and of Priestley, as well as in the more refined and masterly experiments of Berthollet, had left no doubt of its nature on the minds of chemists in general, it was not impossible that a small quantity of water in their results might have been overlooked, when dissolved in the gases or deposited on the vessels employed, or might even be neglected, as not arising from elements essential to the construction of the ammonia. His conjecture was confirmed in the first place by the formation of carbonic acid in the purest and driest ammoniacal gas, by means of charcoal intensely ignited by a voltaic battery.

When Mr. Davy had afterwards exposed the base of potash to ammoniacal gas, and had ascertained the formation of potash, no doubt was left of the *presence* of oxygen, although its *proportion* could not by these means be ascertained. For the purpose of determining the proportion, and at the same time of avoiding any fallacy that might be occasioned by the presence of water in the ammoniacal gas, even in its state of hygrometrical dryness, Mr. Davy had recourse to the apparatus employed by Messrs. Allen and Pepys for the combustion of the diamond. The gas, after being carefully prepared and dried, was passed over the surface of iron wire ignited in the platina tube, furnished with an additional apparatus for condensing any water that might be formed in the experiment.

In the course of a quarter of an hour four grains of ammoniacal gas were decomposed. The iron wire had gained $\frac{12}{100}$ths of a grain, and a quantity of water was moreover collected, which weighed full four tenths of a grain. From the amount of the oxygen contained in the water, together with that which caused the increase of weight in the iron, Mr. Davy infers that 100 parts of ammonia contain nearly 20 of oxygen.

We may therefore consider oxygen as existing in, and as forming an essential element of all the three alkalies; so that this principle of acidity, as it is termed in the French nomenclature, might now equally be called the principle of alkalization.

From analogy alone, it is reasonable to presume that the alkaline

earths are compounds of a similar nature; and in some experiments upon both barytes and strontian, inflammable matter was produced at the negative surface, and burned with a red flame. This matter Mr. Davy has much reason to believe was the basis of the earth employed. Moreover, although these earths have the strongest relation to the alkalies, there is also a further chain of resemblances through lime, magnesia, glucine, alumine, and silex; each of which, there is some reason to imagine, may yield new elements when subjected to analysis by electric attraction and repulsion. Nor, indeed, are our hopes or expectations confined to the decomposition of earthy substances; as there is equal reason to suppose that the three acids which have hitherto resisted decomposition, by the usual means of chemical analysis, may nevertheless yield the oxygen which they have been presumed to contain when subjected to the more intense action of electro-chemical affinity.

On the Structure and Uses of the Spleen. By Everard Home, *Esq. F.R.S.* Read November 26, 1807. [*Phil. Trans.* 1808, *p.* 45.]

Mr. Home, in the course of his late investigation of the functions of the stomach, having observed that the pyloric and cardiac portions of that cavity were separated in many animals by a permanent division, and in most by at least a temporary muscular contraction, during the process of digestion; and having also found that the food in the pyloric portion has in general nearly the same consistence, was led to consider by what means the quantity of fluid frequently taken at meals could be absorbed from the cardiac portion; and he imagined the spleen, from its contiguity to the stomach, to be the most natural channel for that purpose.

To ascertain whether liquids do really pass from the stomach by any other channel than the pylorus, that passage was secured by a ligature in a living dog, and five ounces of fluid coloured with indigo were injected into the stomach. At the end of half an hour, two ounces were brought up by vomiting; and the dog being killed, one ounce was found remaining in the stomach; so that two ounces had escaped. The spleen was turgid: and upon making a transverse section of it, the cut surface presented an appearance of molecules or vesicles of a white colour, surrounded by the small ramifications of the arteries and plexuses of small veins.

In Mr. Home's next experiment, he endeavoured to detect the course of the fluid from the stomach of a dog, by employing a decoction of madder; but though the urine appeared to be somewhat tinged with the madder, no such colour could be discerned in the spleen.

At the suggestion of Mr. William Brande, he next substituted tincture of rhubarb, the presence of which might be made very evident by the addition of an alkali, which occasions it to assume a brownish red colour.

After having learned, by various trials, by what time and within what period the effect of rhubarb might be perceived in the urine of

a man, he again made a dog the subject of experiment; and after he had tied the pylorus, about three ounces of a mixture of tincture of rhubarb with water were at intervals injected into his stomach. Upon killing this dog, at the expiration of eight hours and a half, his bladder was found distended with urine highly tinctured with the rhubarb. The spleen was turgid; and, when cut through and examined by a magnifying-glass, appeared to consist of two parts intermixed, but very distinguishable from each other by their colour; the one was transparent, in the form of small circles or ovals, and surrounded by a different structure, which was vascular, of a red colour, but of a lighter hue than the substance of the liver.

This spleen was immersed in water, and being cut into small pieces, the water became discernibly impregnated with the rhubarb, as was rendered manifest by the test of alkali.

On the contrary, an equal portion of the liver of the same dog, treated in the same way, gave no such colour to the water, but only tinged it with blood of a red colour. Although fluids are thus found to pass from the stomach to the spleen, the vessels by which they are conveyed have not been detected, nor does Mr. Home entertain much hope of such a discovery.

On the Composition of the Compound Sulphuret from Huel Boys, and an Account of its Crystals. By James Smithson, *Esq. F.R.S.* Read January 28, 1808. [*Phil. Trans.* 1808, *p.* 55.]

Mr. Smithson gives a particular description of the form of the sulphuret of lead, antimony, and copper, because that which was laid before the Society in 1804 appeared to him materially inaccurate and imperfect; and he further offers some observations upon Mr. Hatchett's experiments, which he deems essentially necessary to our rightly understanding this substance, as well as many other chemical compounds to which the same principles extend.

The author conceives it not to be probable that this ore is a direct quadruple combination of the three metals, lead, antimony, and copper, with sulphur; but thinks it much more credible that it consists of the three sulphurets of these metals.

On this presumption he makes experiments, to determine the proportion of these sulphurets to each other; and since 10 grains of galena produce 12¾ sulphate of lead, he thence infers the quantity of galena indicated by 60·19 grains of sulphate of lead, obtained by Mr. Hatchett. So also with respect to sulphate of antimony;—as 10 grains yield 11 of precipitate from muriatic acid by water, he is enabled to determine the quantity of sulphuret of antimony, corresponding to Mr. Hatchett's precipitate of 28·64 grains. With respect to sulphate of copper, his method is not so direct; for as he had none of this sulphuret on which to make experiments, he only presumes that the remainder of the ore consists of this compound; and hence he obtains the following results: sulphuret of lead, 49·7; sulphuret of antimony, 29·6; sulphuret of copper, 20·7.

From the near agreement of these numbers with the simple pro-

portions of 50, 30, and 20, he thinks it no great violation of probability, to suppose that experiments affected with no error would, in fact, have given these integral results instead of the former decimal parts.

Mr. Smithson proceeds further to express his doubts, not only of the existence of quadruple, but even of strictly triple compounds. He believes that all combination whatever is binary, and is inclined to consider the present compound as consisting of equal parts of galena and fahlertz; the latter being also a binary compound of the sulphurets of antimony and of copper, in the proportion of three of the former to two of the latter.

The author next computes the proportion of the four ultimate elements; and these, being deduced from assumed simple fractions, are simply as the numbers 12, 25, 15, and 8. These, he remarks, are sexagesimal parts of this ore, as were those also which in a former paper he assigned to calamine.

When in that communication he offered a system founded on the results of his own experiments, he is apprehensive that he may have been supposed to be influenced, even unconsciously to himself, by a favourite theory; but the present case he thinks not liable to the same objection, since no fondness for theory affected the experiments of Mr. Hatchett, which nevertheless accord with its principles when viewed in a proper light.

Mr. Smithson, conceiving it established that chemical compounds consist of elements united in simple proportions by weight, observes, that greater accuracy is to be expected from correct theory than can be obtained in chemical experiments.

The principles of his theory require that simple ratios should always obtain in binary compounds; and he gives instances from the subjects of the foregoing experiments, which any chemist can, by careful repetition, confirm.

The ratios which he assigns to the compounds of lead are such, that two parts of lead make three of sulphate of lead, and five of lead make six of sulphate of lead. So also five of antimony make six of sulphuret, and three of antimony make four of powder of algaroth.

From the only crystalline form which Mr. Smithson believes to exist of the triple sulphuret, he infers that its primitive form is a cube, and not a tetrahedral prism, as stated by Count Bournon; and he observes, that the angles given by the Count are at variance with each other.

On Oxalic Acid. By Thomas Thomson, *M.D. F.R.S.Ed. Communicated by* Charles Hatchett, *Esq. F.R.S.* Read January 14, 1808. [*Phil. Trans.* 1808, *p.* 63.]

Though much important information has resulted respecting the *formation* of this acid, from the experiments of Hermbstadt, Westrumb, Berthollet, Fourcroy, and Vauquelin, the *properties* of it have been rather neglected since the original dissertation of Bergman, to whom

we are indebted for the first account of its properties. Dr. Thomson has in consequence undertaken a set of experiments, with the view of ascertaining various particulars respecting it.

Since the crystals of oxalic acid effloresce and lose a part of their weight when moderately heated, he endeavours to ascertain what portion of this loss was to be ascribed to water of crystallization, by uniting a known quantity of the acid with lime, by precipitation from a known solution of it in muriatic acid.

The quantity of acid employed weighed 58·3 grains; the oxalate of lime produced, when perfectly dried, weighed 72 grains. This oxalate being heated to redness, gave 49·5 carbonate of lime; and by a further exposure to a violent heat, yielded 27 pure lime, which being deducted from 72 oxalate, left 45 for dry oxalic acid, or $\frac{77}{100}$ of the quantity employed for saturation. The same experiment also gives the proportion of acid to base in the oxalate of lime to be $\left\{\begin{array}{r} 62{\cdot}5 \\ 37{\cdot}5 \\ \hline 100{\cdot}0 \end{array}\right.$; a proportion which differs from that of Bergman, because he neglected to neutralize the acid from which the lime was precipitated, and which retained a part in solution.

To obviate any chance of error in so fundamental an experiment, Dr. Thomson thought it worth while to verify that analysis by a different mode of operating. A known quantity of acid having been precipitated by lime-water, he obtained a quantity of oxalate of lime that corresponded accurately with the foregoing estimate.

The oxalate of magnesia is very similar to that of lime, and is not sensibly dissolved by water; nevertheless, if a solution of oxalate of ammonia be poured into a solution of sulphate of magnesia, no precipitate is formed till after concentration by heat.

Oxalate of potash readily crystallizes in flat rhomboids, which dissolve in thrice their weight of water at 60°. This salt also combines with excess of acid, forming a superoxalate, long known by the name of Salt of Sorrel, very sparingly soluble in water. The potash in this salt, as Dr. Thomson remarks, contains very nearly the double of that quantity of acid which would be necessary barely to neutralize it.

Soda also forms, with this acid, a salt that readily crystallizes, and it is said to be capable of combining with excess of acid; but Dr. Thomson has not tried it.

The oxalate of ammonia is much less soluble than either of the preceding. Dr. Thomson having carefully examined, by direct saturation of oxalic acid, the proportions in which the acid and base unite to form the several earthy and alkaline oxalates, gives tables of them, adapted to various practical purposes: but having remarked that oxalate of strontian thus formed contained a larger quantity of the earth than was expected, he neutralized a known quantity of oxalic acid by ammonia, and with that compound made a precipitate from muriate of strontian. By this method of obtaining the compound, the same quantity of acid was found to have united with only half the quantity of strontian that had been contained in the former precipitate; a

proportion which he had before observed to take place in the super-oxalate and neutral oxalate of potash.

In the decomposition of these salts by heat, Dr. Thomson found the acid to be resolved into water, carbonic acid, carbonic oxide, carburetted hydrogen, and charcoal.

With the view of determining with precision the composition of oxalic acid, Dr. Thomson made choice of the oxalate of lime, of which 100 grains by distillation yielded 60 cubic inches of gas, consisting of carbonic acid gas and inflammable gas, in the proportion of 2 of the former to 7 of the latter. The inflammable gas also consisted of 2 parts, seven tenths being carbonic oxide, and three tenths carburetted hydrogen.

Hence if 160 grains of oxalate of lime, which contain 100 oxalic acid, be distilled, the products are, 59·53 carbonic acid, 24·28 inflammable air, 11·51 water, 4·68 charcoal: and as the constituents of these products are known, the ultimate elements are, 64·69 oxygen, 31·78 carbon, 3·53 hydrogen; which Dr. Thomson considers to be, 64 oxygen, 32 carbon, and 4 hydrogen.

In the analysis given of this acid by Fourcroy, as performed by Vauquelin and himself, the quantity of carbonic acid appears much too small; and Dr. Thomson is convinced their method must be erroneous, as the quantity of carbonic acid alone that is formed during distillation contains considerably more carbon than they assign to oxalic acid.

From the weights of the elements obtained from oxalic acid by chemical analysis, Dr. Thomson turns to views of a different nature, and hopes to arrive at a more intimate and accurate knowledge of the difference between this acid and other vegetable products consisting of the same ingredients, by attending to certain numerical relations of their elements to each other: and this relation is such, that if hydrogen be expressed by 1, the number which corresponds to carbon is 4·5, and oxygen 6. Azote, expressed according to the same scale, will be 5. The law observable in their union is this, that in all their compounds the proportions of these constituents may be always expressed by these numbers, or by small multiples of them; for instance,

	Oxyg.	Hydr.	Carb.	Azote.
Water consists of	6	1	—	—
Carbonic oxide	6	—	4·5	—
Carbonic acid........	2×6	—	4·5	—
Carburetted hydrogen..	—	2×1	4·5	—
Olefiant gas	—	1	4·5	—
Nitrous oxide........	6	—	—	2×5
Nitrous gas..........	6	—	—	5
Nitrous acid	2×6	—	—	5

From the knowledge of this law, which was first observed by Mr. Dalton, it is difficult (says Dr. Thomson) to avoid concluding, with him, that the numbers above given represent the relative weights of a single atom of each of these elements; that they first unite atom

to atom; but that they may also combine in the proportion of two or more particles of one sort with one of another.

Dr. Thomson observes, that the same law holds also with respect to salts, and that numbers may be affixed to each of the acids and to each of the bases; which numbers, or their multiples, will represent them in all the combinations into which these bodies enter.

In this scale the particle of sulphuric acid is represented by 33, muriatic acid by 18, nitric acid by 17, carbonic acid by 17·5, barytes 67, lime 23, soda 24, ammonia 6.

From these data, and from the proportion in which oxalic acid has been found above to combine with several bases, Dr. Thomson assigns the number 39·5, which represents the particle of oxalic acid. Reverting next to the proportion of its elements, and to the weights of their respective atoms, he finds the integrant particle of oxalic acid to consist of 4 atoms of oxygen, 3 of carbon, and 2 of hydrogen; the aggregate weights of which amount to the same number, 39·5, at which he had arrived by a different mode of estimation.

According to these proportions, 100 parts of oxalic acid will consist of its three elements, in the proportion of 61, 34 and 5, instead of 64, 32 and 4; numbers not exactly corresponding, but, in the estimation of Dr. Thomson, approaching sufficiently near to heighten the probability of the reasoning employed.

We may next conceive 3 particles of oxalic acid thus constituted to be decomposed at once, and to yield 4 particles of carbonic acid, 2 of carburetted hydrogen, and 2 of carbonic oxide, 3 of water, and 1 particle of charcoal; and might thence expect 100 parts of acid to yield,

Carbonic acid	55·70,	instead of	59·53,	actually obtained.
Inflammable gas ..	28·64,	——	24·28,	——
Water	11·81,	——	11·51,	——
Charcoal	3·80,	——	4·68,	——

It is impossible, Dr. Thomson observes, to expect exact correspondence till the numbers representing the weights of the elementary atoms be ascertained with accuracy, instead of the round numbers which he has assumed, for the purpose of showing an approximation of the theoretic inferences to the results obtained by experiment.

In an analysis of sugar, which follows, by a series of experiments and of hypothetical reasoning, different from the experiments and reasoning of Lavoisier, Dr. Thomson nevertheless agrees with him, to great accuracy, in his results: and assuming 64 oxygen, 8 hydrogen, and 24 carbon, as the true elements, if these numbers be respectively divided by the weights of their single particles, the number of atoms of each which combine to form sugar are to each other as 5, 3, and 4 respectively.

On Super-acid and Sub-acid Salts. *By* William Hyde Wollaston, *M.D. Sec. R.S.* Read January 28, 1808. [*Phil. Trans.* 1808, *p.* 96.]

The author having some time since observed various instances of super-acid salts, in which he found that the quantity of acid was exactly double of what would be necessary merely to saturate the alkali present, had intended to have pursued the inquiry, but was prevented by the publication of Mr. Dalton's theory of chemical combination, as explained by Dr. Thomson.

However, since it appeared that Mr. Dalton's theory was originally founded on the analysis of gases, which is attended with too many difficulties to be repeated by every one who may be desirous of verifying the law of simple multiples by experiment, the author thought it might be desirable to select from his observations certain experiments extremely easy to be repeated, each of which exhibits in itself the most direct and simple proof of the law under consideration.

The subcarbonates of potash and of soda are named as instances of salts containing exactly half the acid necessary to form their fully-saturated carbonates; and the proof is obtained by taking two quantities of either of these alkalies fully carbonated. One quantity being taken exactly double of the other, is exposed to a red heat, and is thereby reduced to a semi-carbonate; for it now yields the same measure of gas that is given out by the unburnt half quantity.

Super-sulphate of potash is adduced as an instance of a salt containing an exact double share of acid. The proof consists in taking two equal quantities of potash, to one of which is added any large quantity of sulphuric acid, and the mixture is exposed to a red heat, by which a part of the redundant acid is expelled; but such an excess of acid remains as is neutralized by the second equal portion of alkali reserved for that purpose.

Super-oxalate of potash is also proved to be an exact binoxalate. Two equal quantities are taken of salt of sorrel; one is exposed to a red heat, to destroy the whole of its acid, after which the alkali that remains is just sufficient to neutralize the redundant acid of the other portion.

In addition to the preceding compounds selected as distinct examples of semi-acid and bin-acid salts, the author has observed one remarkable instance of a more extended and general prevalence of the same law. For when the circumstances are such as to admit the union of a further quantity of oxalic acid with potash, the quantity is again doubled; so that the potash present is sufficient to saturate only one part in four of the acid contained in any quantity of the salt; and, consequently, the three redundant parts of acid require for their neutralization the alkali of three equal quantities of this quadroxalate, which may be obtained by burning, as proof that the proportion has been rightly ascertained.

The last experiment is designed to determine whether potash would

also unite with three quantities of oxalic acid; but it was found, that when two parts of potash are in solution with six equivalent quantities of oxalic acid, they do not crystallize together in this proportion; but one part of the potash becomes a true binoxalate by union with two parts out of the six of oxalic acid taken; and the other part of potash is found united with the remaining four parts of acid.

The author expresses an opinion that we shall not be able to explain satisfactorily, why this acid refuses to unite in the proportion of 3 to 1, till we can attain a just conception of the geometrical arrangement of the elementary particles in all the three dimensions of solid extension. It being supposed, for instance, that the particles are spherical (which is the simplest hypothesis), if they unite 1 to 1, there is but one mode of union. If 2 particles are united to 1, the 2 particles will arrange themselves at opposite poles of that to which they are united. If there be 3 particles, the only regular position in which they could remain is in the form of a triangle in a great circle surrounding the single spherule; but for want of similar matter at the poles of this circle, the equilibrium would not be stable. But again, if there be 4 to 1, a stable equilibrium would occur, when they assume the form of a regular tetrahedron, surrounding the single particle.

But as the author does not place much reliance on this explanation, since such a geometrical arrangement of the primary elements is altogether conjectural, he is desirous that it should not be confounded with the results of the facts above related, which are distinct and satisfactory with respect to the existence of the law of simple multiples.

On the Inconvertibility of Bark into Alburnum. By Thomas Andrew Knight, *Esq. F.R.S. In a Letter to Sir* Joseph Banks, *K.B. P.R.S.* Read February 4, 1808. [*Phil. Trans.* 1808, *p.* 103.]

Mr. Knight having on a former occasion observed the bark of trees to originate from a fluid exuding from both bark and alburnum, continues the subject by observations, tending to prove that bark thus formed always remains in the state of bark, and that no part of it is ever converted into alburnum, as various eminent naturalists have maintained.

Equal portions of bark from several branches of an apple and a crab-tree were removed by circular incisions, and transposed from tree to tree in the spring; and a vital union was secured by bandages, and by a plaster of bees' wax and turpentine.

When some pieces of bark were removed in the autumn of the same year, a layer of alburnum was found to have been formed beneath them in every instance; that of the crab-tree having the colour and roughness of the stock on which it was produced, while that of the apple-tree showed none of the sinuosities of the bark of the crab-tree which covered it; neither did the vessels and fibres of the newly-generated alburnum in any degree correspond with those of the trans-

posed bark. And it was evident, that in each instance a new layer, both of cortex and of alburnum, was generated.

Mr. Knight's attention was next directed to the progressive formation of alburnum in the young shoots of an oak coppice; but he could discover nothing like transmutation of bark into alburnum, although the commencement of alburnous layers in this tree is peculiarly conspicuous, by a circular row of very large tubes. These tubes he found, at their first formation, passing through a soft gelatinous substance, much less tenacious than the surrounding preexistent bark; and there was nothing in the bark at all corresponding to the circular row of tubes contained in the alburnum. The interior surface of the bark is at the same time well defined, and its own peculiar vessels are distinctly visible, and by no means exhibit any appearance of progressive transmutation.

Mr. Knight remarks also, that the qualities of different kinds of wood are not in any degree indicated by the bark which covers them. He instances the wych-elm and the ash, the woods of which, for agricultural implements, are frequently substituted for each other, although the textures of their barks are extremely dissimilar; inasmuch as one is brittle, and the other so tough as frequently to be used for ropes.

Another circumstance, very unfavourable to the theory of conversion, is the firm adhesion which subsists between the layers of bark to each other, in comparison to their adhesion to their alburnum.

Two experiments of Du Hamel are, however, cited by Mirbel in support of that theory.

In the first, pieces of silver wire, inserted into the bark, were frequently found in the alburnum; but the evidence is defective, as it was not rightly ascertained that the pieces of wire did not, at their first insertion, pass between the bark and alburnum, and thus be liable to be covered by a new deposition of either one or the other.

In the second experiment, the bud of a peach-tree, with a piece of bark attached to it, was inserted into a plum-stock: a layer of wood was afterwards found beneath the inserted bark, perfectly similar to the peach; but it is easier to conceive a layer of alburnum, generated by deposition from fluids that have circulated through the inserted bud, than that a part of its bark should be converted into a layer of alburnum more than twice as thick as the inserted bark.

Mr. Knight also remarks, that when the bud is destroyed, the bark deposits no alburnum; but, being small, it becomes ultimately covered by the successive alburnous layers of the stock, and may be found many years afterwards to have made no progress towards conversion into wood.

Some Account of Cretinism. By Henry Reeve, *M.D. of Norwich. Communicated by* William Hyde Wollaston, *M.D. Sec.R.S.* Read February 11, 1808. [*Phil. Trans.* 1808, *p.* 111.]

The peculiar idiots, Cretins, which prevail in the Valais and in Switzerland, appear to have been first described by Felix Plater in 1656; but the author of the present communication refers to M. de Saussure for the most accurate account of the appearance of the disorder; to Malacarni of Turin, and to Professor Ackerman, for a particular description of several cretins that they dissected; and for a more full account of the malady, to the *Essai sur le Goitre et Cretinisme*, by Mons. Foderé.

By these and other accounts, Dr. Reeve's curiosity was excited to examine what connexion could subsist between mental imbecility and that enlargement of the thyroid gland which so frequently accompanies it, and which, in general, first attracts the notice, and has much occupied the attention, of those who have described cretinism. But this is not a constant attendant; while the more essential symptoms are, deformed head, diminutive stature, a sickly complexion, a vacant countenance, coarse and prominent eyelids, wrinkled and pendulous skin, with muscles proportionally flabby. The qualities of the mind correspond to the outward appearance, and vary in all degrees, from common stupidity to complete fatuity.

Notwithstanding the assertions and ingenious reasoning of Foderé and other authors upon the supposed connexion between goitre and cretinism, the author is inclined to think, from the instances which he had an opportunity of observing in the neighbourhood of Martigny, that the two disorders are perfectly distinct; and that though they frequently occur in the same person in countries where both disorders are endemial, yet no necessary connexion subsists between them: for even there, many persons have goitre without cretinism, and many cretins have no goitre; and in Britain, we know that no weakness of the intellectual powers accompanies bronchocele.

Dr. Reeve is inclined to ascribe this singular malady to peculiarities in the physical constitution of certain districts. The valleys where cretinism is most frequent, as Saussure justly observes, are surrounded by very high mountains; they are sheltered from currents of air, and are exposed to both the direct and reflected rays of the sun. The atmosphere is humid, close, and oppressive: the houses of the cretins are also generally in the most confined situations, are very filthy, very hot, and miserable habitations; while in the more airy and elevated parts of the mountains, no cretins are to be seen.

The hypothesis, that snow-water is the cause of goitre and of cretinism, our author observes, is contradicted by the most obvious facts; since there are many places contiguous to glaciers where the inhabitants can drink nothing but snow-water, and yet are not subject to these disorders; which, on the contrary, do occur in some places where snow-water is unknown.

The theory also which ascribes them to waters impregnated with

calcareous matter is equally unfounded; and even the general opinion, that goitre is endemial in mountainous countries, is of no value, since it is rare in Scotland, though mountainous, and very common in the county of Norfolk.

From those dissections which have been made of cretins by Ackerman, by Malacarni of Turin, and by Foderé, some very singular appearances in the cranium have been observed. There was no cavity for the reception of the pons varolii and medulla oblongata; and that which contained the cerebellum scarcely exceeded one third of its natural capacity.

The present paper is accompanied with two drawings taken in the anatomical museum at Vienna, from the skull of a cretin who died at thirty years of age; yet the fontanelle is not closed, the second set of teeth are not out of their sockets, and none of the bones are distinctly and completely formed. Every part bears marks of irregularity in the growth and formation of bone. The zygomatic and maxillary processes of the ossa malæ are wanting; the ossa nasi very small; in the temporal bone the zygomatic process terminates at the coronoid process of the lower jaw; the mastoid and styloid processes are wanting; the os occipitis is unusually large, and numerous additional ossa triquetra are seen along the whole course of the lambdoidal suture.

Cretinism, says the author, is a most distinct instance of the effect of physical causes on the intellectual as well as on the bodily powers; and it is now sufficiently ascertained, that it may be prevented by removal of children from the confined and dirty situations to the more open and airy parts of the mountains: and, accordingly, the number of cretins has, within the last ten years, sensibly diminished. The analogy between this disorder and rickets is considerable. It is remarkable, that they were both first described nearly at the same time; and it is to be hoped that they will disappear together, and at some happier period be known only by description.

On a new Property of the Tangents of the three Angles of a Plane Triangle. By Mr. William Garrard, *Quarter Master of Instruction at the Royal Naval Asylum at Greenwich. Communicated by the Astronomer Royal.* Read February 11, 1808. [*Phil. Trans.* 1808, *p.* 120.]

On a new Property of the Tangents of three Arches trisecting the Circumference of a Circle. By Nevil Maskelyne, *D.D. F.R.S. and Astronomer Royal.* Read February 18, 1808. [*Phil. Trans.* 1808, *p.* 122.]

The same property which at the last meeting was stated by Mr. Garrard to belong to the tangents of any three parts of a semicircle, was in this paper extended to all cases of trisection of the whole circle; but the demonstration of course could not be read to the Society.

An Account of the Application of the Gas from Coal to economical Purposes. By Mr. William Murdoch. *Communicated by the Right Hon. Sir* Joseph Banks, *Bart. K.B. P.R.S.* Read February 25, 1808. [*Phil. Trans.* 1808, *p.* 124.]

An apparatus for the production of the gas from coal having been prepared by Mr. Murdoch, for the very extensive cotton manufactory of Messrs. Philips and Lee, at Manchester, which is now illuminated by means of this alone upon a very large scale, this instance was selected as the best for estimating the expense of employing the gas lights.

The quantity of light there employed was ascertained by comparison of the shadows to be equal to that of about 2500 mould candles of six in the pound, each of which consumes about four tenths of an ounce of tallow per hour.

The coal is distilled in large iron retorts, and the gas conveyed by pipes of iron to large reservoirs or gasometers, where it is washed and purified before it is conveyed to the mill. The main pipes branch off into a variety of ramifications (the aggregate length of which amounts to several miles), the several branches diminishing in diameter in proportion as the quantity of gas to be passed through them becomes less.

The burners where the gas is consumed communicate with the main by short pipes, furnished each with a cock to regulate the admission of gas. These burners are of two kinds: one is on the principle of the Argand lamp, and the other has a conical termination with three holes, one at its point, and the other two placed laterally about one thirtieth of an inch in diameter. The former, of which there are 271, are each equal to four candles; and the latter amounting to 633, are each equal to $2\frac{1}{4}$ candles; so that the total amount is, as above stated, about equal to 2500 candles.

For the hourly supply of these burners, 1250 cubic feet of gas are necessary; and since the lights are used for about two hours, the daily consumption of gas is 2500 cubic feet.

For the production of this quantity of gas, seven hundred weight of cannel coal is employed, which, notwithstanding its high price, is found to be the most economical, on account of the superior quality and quantity of the gas it yields. The number of working days being 313, the annual consumption of cannel coal is 110 tons, of which the cost is 125*l.* But these 110 tons yield 70 tons of coke, the value of which is 93*l.*, leaving a difference of 32*l.*, to which must be added 20*l.* for the value of 40 tons of good common coal employed for heating the retorts.

But by far the greatest part of the cost of employing this species of light consists in interest of capital employed in furnishing the apparatus, and in repairing the wear and tear, which are stated together by Mr. Lee at about 550*l.* per annum, making a total annual expenditure of 600*l.* instead of 2000*l.*, which would be required to produce an equal quantity of light from 2500 candles burning together, at 1*s.* per pound.

If the estimate were made upon three hours instead of two per day, it is evident that the comparison would be still more in favour of the gas lights, since the interest of capital would be the same, and the wear and tear not much greater; so that the annual cost might be about 650*l*. instead of 3000*l*.

The introduction of the gas lights into this manufactory has been gradual: at first some inconvenience was experienced from the smell; but this objection has been wholly removed by improved methods of purifying the gas, and it is now much approved by the work-people for the perfect steadiness of the light; and it is wholly free from the inconvenience of snuffing, and from the danger occasioned by sparks that fall from candles.

In addition to the foregoing statement of comparative economy, the author conceives it may be interesting to the Society to be informed of the original application of this gas, as a substitute for oil and tallow, which he states to have put in practice nearly sixteen years, in consequence of experiments which he was at that time conducting at Redruth, in Cornwall, upon the distillation of various mineral and vegetable substances.

It was not, however, till the year 1798, that he removed from Cornwall to the manufactory of Messrs. Boulton and Watt, at the Soho foundery, and there constructed an apparatus on a large scale, for the purpose of lighting their principal building. Since that period it has been extended to the greatest part of their manufactory, to the exclusion of other artificial light; but Mr. Murdoch has preferred collecting his estimate from the apparatus of Messrs. Philips and Lee, on account of the greater extent and greater uniformity of the lights.

Although the author did not derive his information concerning the inflammability of this gas from any source but his own experiments, he has since learned that "the inflammable spirit of coals" is mentioned by Dr. Clayton in the forty-first volume of the Philosophical Transactions, so long since as the year 1739; and he is informed that the current of gas escaping from Lord Dundonald's tar-ovens had been frequently set on fire previous to the date of his experiments: but he thinks himself entitled to claim the original idea of applying it as an economical substitute for oils and tallow for the purpose of illumination.

Further Experiments on the Spleen. By Everard Home, *Esq. F.R.S.* Read February 25, 1808. [*Phil. Trans.* 1808, *p.* 133.]

The author having established by the experiments which he lately communicated to the Society, that when the pylorus is closed by a ligature, fluids pass from the stomach into the circulation through the medium of the spleen, has since that time conducted a new course of experiments to determine whether there is the same passage also in the natural state of these parts.

Six asses were the subjects of as many experiments. To the three

first, tincture of rhubarb was given, to the quantity of a pint and a half in three doses of half a pint each, with the same quantity of water. The fourth and fifth had powdered rhubarb made into a bolus, and the sixth took three pints of infusion of rhubarb.

When the asses were killed, equal quantities of blood were taken from the splenic vein and from the left auricle of the heart, or from the vena cava, and suffered to coagulate, that the serum of each might be obtained for examination by alkalies, in comparison with each other, and with the urine of the animal, as well as with infusions of its spleen and of its liver in water.

In the first of the experiments with tincture of rhubarb, the infusion of the spleen had a tint of colour equal in intensity to that of sixty drops of tincture of rhubarb in two ounces of water; the serum from the blood of the splenic vein, to fifteen drops; the serum from the auricle, to three drops. The urine had so deep a tinge that it nearly resembled the pure tincture itself.

In the second and third experiments the results were nearly similar, but less intense. But in those asses to which the rhubarb boluses had been given without any fluid, the spleen was found in its contracted state, with cells scarcely visible, and without sensible impregnation by the rhubarb; but the cæcum and colon contained several quarts of fluid, in which the rhubarb was more evident both to sight and smell than in the stomach. The urine also was highly impregnated with the colour of the rhubarb. The effects from infusion of rhubarb were perfectly similar to those from the tincture, but the colours occasioned by it were not so intense.

In the course of these experiments, an attempt was made to ascertain whether the blood from the splenic vein contained more serum than that from other parts of the body; but the difference observable was not so great as it was afterwards found might be occasioned by other circumstances.

From the experiments contained in his former and present paper, Mr. Home considers it ascertained that the spleen is sometimes found distended to double the bulk which it occupies in its more contracted state.

In the distended state there is a cellular structure distinctly visible, but in the contracted state these cells cannot be seen without a magnifying-glass; the difference between these states depending upon the quantity of liquid that was contained in the stomach before death.

If the fluids contained in the stomach be coloured with tincture of rhubarb, the spleen and the blood in the splenic vein are coloured also, more strongly than the liver or blood contained in other veins of the body; so that the colour cannot arrive at the spleen through the ordinary course of the circulation. But when the stomach is kept without liquids, although the colouring matter be carried through the system to the urine by the ordinary channel, no particular evidence of it is to be met with in the spleen or its vessels; but the principal absorption takes place from the cæcum and colon. No vessels, however, have been discovered by which the communica-

tion between the stomach and spleen is carried on; but the evidence is too strong to leave a doubt on the mind of the author as to their existence.

Observations of a Comet, made with a View to investigate its Magnitude and the Nature of its Illumination. To which is added, an Account of a new Irregularity lately perceived in the apparent Figure of the Planet Saturn. By William Herschel, *LL.D. F.R.S.* Read April 7, 1808. [*Phil. Trans.* 1808, *p.* 145.]

Dr. Herschel presuming that the motion of the comet would be correctly ascertained at the Royal Observatory at Greenwich, confined his observations to its physical condition, and relates the several circumstances which he remarked concerning its nucleus, its head, its coma, and its tail. The form of its nucleus was particularly attended to on the 4th of October, and remarked to be perfectly circular and equally bright on all sides.

Its magnitude was at the same time estimated at about 3″, due care being taken to determine that the visible disc was not spurious, by means described in a former paper. It was examined with a 10-feet reflector, and with powers of 200 and 300; but its light was not found sufficient to bear higher powers.

On the 19th at 5^h 40′ it was again estimated at $2\frac{3}{4}$″, but at 6^h 20′ it was compared with Jupiter's third satellite, and found to be rather less than it.

The coma or nebulous appearance surrounding the head was at the same time estimated at about six minutes, and on the 6th of December at 4^h 45′.

The tail on the 18th of October measured $3\frac{3}{4}$°, but on the 6th of December it was reduced to 23′. The tail was at various times observed to appear longer and more distinct on the south preceding, than on the north following, side.

Various authors having said that the tails of comets were so rare as not to affect the light of the smallest stars seen through them, Dr. Herschel paid particular attention to that circumstance; but uniformly found those stars which he saw emerge from behind the tail to become brighter than before in comparison to neighbouring stars.

On the 6th of December the head had the appearance of a large brilliant round nebula, suddenly much brighter in the centre. On the 1st and 14th of January it had still more the appearance of a nebula, with an increase of brightness in the middle; but even so late as the 21st of February, the nebulosity was observed to project a little on that side where the tail used to be seen.

The inferences which Dr. Herschel draws from these observations, are, first, that the body of the comet consists of solid matter about 538 miles in diameter; secondly, that since the portion of its visible hemisphere which could be illuminated by the sun on the 4th of October did not exceed 120°, and since it did not appear gibbous but perfectly circular, the surface of the comet must itself be luminous,

and the same inference is drawn respecting the nature of the tail; for if it were sufficiently dense to be visible by reflected light alone, at the distance of 235 millions of miles, its opacity would entirely prevent our seeing stars through it.

The length of the tail is computed to have been on the 18th of October upwards of nine millions of miles.

The resemblance of this comet to a nebula during the last ten weeks of its appearance excites a suspicion in Dr. Herschel's mind, that he may possibly have classed as nebulæ other cometary bodies; but it would be a task of too many years' labour to revise his catalogue of nebulæ for the chance of discovering any deficiency of those formerly observed.

In the second part of this paper, Dr. Herschel informs us, that he has remarked a new irregularity in the apparent form of Saturn; for that in the month of June last, there was a visible protuberance of its south pole, which could not have been overlooked at the time of his former observations. This he ascribes to the refraction of light in its passage through the atmosphere of the ring, which was interposed between us and the southern hemisphere, but, passing behind the northern hemisphere, did not occasion a similar protuberance.

Hydraulic Investigations, subservient to an intended Croonian Lecture on the Motion of the Blood. By Thomas Young, *M.D. For. Sec. R.S.* Read May 5, 1808. [*Phil. Trans.* 1808, *p.* 164.]

In the present inquiry, Dr. Young undertakes to investigate minutely and comprehensively the motion of fluids in pipes as affected by friction; the resistance occasioned by flexure, the laws of propagation of impulses through fluids contained in elastic tubes, the magnitudes of pulsations in conical vessels, and the effect of progressive contraction along a canal;—the physiological application of the results being reserved for a future opportunity.

In the first section the friction and discharge of fluids through pipes are considered; and the author assents to the encomiums bestowed on Mr. Dubuat, by Professor Robison, and other late authors on hydraulics, for his skill in adapting a formula to express the results of numerous experiments on this subject. But since the form of his expressions is not so convenient for practice as might be wished, and fails altogether in its application to extreme cases, Dr. Young has by approximation arrived at a formula, which appears to agree fully as well as Dubuat's with Dubuat's own experiments, which accords better with those of Gerstner, and extends also with equal accuracy to all extreme cases in which the former was erroneous.

In considering the velocities of water flowing through pipes, the friction appeared to consist of two parts, one of which is most apparent in small tubes, and varies as the velocity simply, and the other as the square of the velocity.

In order to show the agreement of Dr. Young's formulæ with the results of experiments, a table is formed containing forty experiments,

taken from Dubuat and Gerstner, with some of his own, the results of which are compared with the velocities calculated according to the *formulæ of Dubuat and of the author.*

The next section treats of the resistance occasioned by flexure of the channel. In this case Dubuat directs the squares of the sines of the angles of flexure to be added together and multiplied by the square of the velocity, and considers the quantity thus obtained proportional to the height necessary for overcoming the resistance. But since the magnitude of this quantity is evidently dependent on the number of parts into which the angle is arbitrarily divided, the author prefers attending merely to the aggregate angle of flexure as expressed in degrees to which the resistance is proportional, but varies also inversely as the radii of curvature, or more nearly as that power of the radius which is expressed by $\frac{7}{8}$. A table which follows shows the comparative correctness of the author's formula with that of Dubuat.

Dr. Young next considers the propagation of impulses through tubes, the elasticity of which supplies the want of elasticity of the fluid contained, and admits the same mode of reasoning that is employed in the case of elastic fluids or solids; for if the elastic force of the tube be as the increase of its circumference, a certain finite height may be assigned, which would cause infinite extension, and which may be called the modular column. The velocity of an impulse at any point will be equal to half that which is due to the height of this point above the base of such a column, and hence the time of ascent of an impulse will be twice as great as that of a falling body; and if the pipe be inclined, the ascent of an impulse will bear the same relation to that of a body moving along an inclined plane.

The magnitude of diverging pulsations is next examined, and the conclusions of Euler, Lagrange, and Bernouilli, who have demonstrated that the velocity of each particle of an elastic fluid is as its distance from the centre of impulse, are supported by a new method of considering the subject.

When a wave is reflected from two surfaces distinctly opposed to each other, they evidently sustain equal pressures; and if to one of these surfaces two others be opposed converging at the acute angle, the wave will be elevated higher as it approaches the angle; and if the height be supposed in the inverse subduplicate ratio of the corresponding subtense of the angle, the pressure will then be equal to that upon the single surface opposed: and hence is an additional reason for inferring, that in all transmissions of impulses the intensity is in the inverse subduplicate ratio of the extent of parts collaterally affected, and this in conformity to the law of the ascending force; but in the case of intersecting waves, there is observed to be a paradoxical deviation, which is deserving of further consideration.

From considering the effect of bodies moving along an open canal, the author infers, that by means of a contraction moving progressively along an elastic pipe, the quantity of fluid impelled will be very

small, unless the contraction be very great in proportion to the diameter of the pipe.

A Letter on the Alterations that have taken place in the Structure of Rocks, on the Surface of the basaltic Country in the Counties of Derry *and* Antrim. *Addressed to* Humphry Davy, *Esq. Sec. R.S. By* William Richardson, *D.D.* Read March 17, 1808. [*Phil. Trans.* 1808, *p.* 187.]

The general design of this paper is to show the great distance to which the same strata may be found to extend, or to have extended, over the surface of a country, and thereby to explain the existence of small detached portions of the same species of matter at considerable distances from each other, as having been originally connected by continuity of the same material over the whole surface of the country, whatever be the present interval, and whatever be the quantity of matter which such an hypothesis supposes to have been removed.

The basaltic area which comprehends most part of the county of Antrim and a portion of Derry, appears to Dr. Richardson peculiarly favourable to such speculations, uncommonly regular in its stratification, and highly favoured by nature in the frequent exposure of the strata in their abrupt and precipitous terminations.

In the island of Rathlin, more especially, the original features are most happily displayed, and are still in good preservation.

It is in the periphery at the northern side that the sections are seen to the greatest advantage, as the perpendicular façades are often continued for miles together.

Of these façades, four are more distinguished for their grandeur and beauty than the rest,—Magilligan, Cave Hill, Fairhead, and Bengore. The two former are the extreme points of the N.W. and S.E. diagonal, and are forty miles asunder; at the summits of mountains, accessible by land. The two latter are visible only from the sea, but are more diversified and more curious in their structure.

The promontory of Bengore, which is nearest to the place of Dr. Richardson's residence, has principally engaged his attention; and the minute description of its strata forms a considerable portion of his memoir, for the purpose of showing the station which the Giant's Causeway occupies in the arrangement of the promontory, and also for the purpose of noticing several facts, which he thinks likely to throw light upon the operations performed on our globe since the *period* of its consolidation.

In the order of the description, Dr. Richardson first gives a general sketch of the promontory when approached from the west, with an account of the inclination, ascent, culmination, and dip of its strata. Of these he enumerates as many as sixteen, and observes that these are all cut perpendicularly in eleven different places by those mighty walls called in Scotland whyn dykes.

These all reach from the top of the precipice to the water, out of

which some of them again emerge in considerable masses, at a distance from the precipice. The dykes are all constructed of horizontal prisms, which form a strong contrast with the vertical pillars of the strata they intersect; and yet, says Dr. Richardson, it is but lately that these singular productions have been noticed; and he states that he himself was the first who observed them, and gave an account which was published in the Transactions of the Royal Irish Academy.

For the purpose of giving a more accurate idea of these dykes, Dr. Richardson gives two views of one of them, which he had overlooked at the time that his essay upon that subject was published.

Of the strata which form the façade of the precipice, the first is sixty feet in thickness, and consists of large basaltic pillars very rudely formed, but generally extending from one surface of the stratum to the other.

The second consists of a substance red as brick, and about nine feet in thickness.

The third is above fifty feet thick, and consists of that variety of basalt termed *irregular prismatic*, resembling in its grain the columnar basalt, but differing in its construction; as its prisms are small, not articulated, unequal and irregular in the vertical or inclined position of their axes.

The fourth is about seven feet thick, columnar but not regular, generally appearing white from a covering of *Byssus saxatilis*, which has a singular predilection for this stratum.

The fifth is ochreous, but less red than other strata of similar consistence; this is very friable, and is generally covered by a coating of grass.

The sixth is composed of rude massy pillars, very coarsely formed, ten feet long. The transition from these to the seventh Dr. Richardson compared to a hand held downwards, and dividing into separate fingers; since the rude columns of the sixth appear continued into those of the seventh, without any distinct line of separation, but are there found divided into neat slender pillars fifty-four feet in length.

The eighth stratum is also fifty-four feet in thickness, and consists of an irregular prismatic basalt similar to the third.

The ninth is that stratum which has attracted principal attention from the beautiful assemblage of neat pillars of which it consists, and which at the intersection of this stratum have been so long distinguished by the name of the Giant's Causeway. This stratum is forty-four feet in thickness; it first appears at its opposite extremity in Portmoon Bay; from hence it rises and culminates between Bengore and Pleskin, with its lower edge 200 feet above the water: in its descent at Noffer it composes the group of pillars called the Organ; at all points wherever it is accessible it has the same grain, with the same size and neatness of its prisms.

The tenth stratum on which these pillars rest, is red as minium, and makes a conspicuous figure from the brightness of its colour.

The six remaining strata are all similar to each other, consisting of tabular basalt, but differing in thickness; and being separated only by thin ochreous layers, the division is not always discernible.

From a revision of the various circumstances observable in these strata, Dr. Richardson selects certain facts which he considers of importance to geology. First, that every stratum preserves the same thickness throughout its whole extent. Secondly, that this uniformity of thickness is interrupted only where the upper surface has been exposed by removal of the superior strata. Thirdly, that the curvature of the summits of the façades does not correspond in form to the surfaces of the strata underneath. Fourthly, that the same arrangement does not continue for more than two or three miles. Fifthly, that wherever materials of different species are in contact, the line of demarcation is always distinct and well defined. Sixthly, that the upper part of any façades, where the strata are exposed, is generally perpendicular, and the lower steep and precipitous. Seventhly, that the rude masses which appear in the sea at the base of the precipices, are not, as has been supposed, ruins which have fallen from the strata above, but are remnants of lower strata remaining in their original position. Eighthly, that these abrupt sections are by no means confined to the coast, but are often formed on the ridges of the hills, at a distance from the sea. Ninthly, that in all such abrupt terminations of strata, whether on the coast or within land, the materials broken off are completely carried away, without a fragment being left behind.

The formation of these abrupt precipices has been, by some, ascribed to the action of the sea: but it is only by careless observers, in Dr. Richardson's estimation, that such an hypothesis can be admitted; since even here the base of that part, which is perpendicular, is elevated 200 or 300 feet above the level of the sea; and the bases of others are no less than at 1400 feet elevation, and at the distance of four miles from the sea at Magilligan, of thirteen at Bienbraddock, and seventeen miles at Monyneeny.

The exact resemblance between the inland façades and those on the shore, proves them all to have been cut down perpendicularly by the same agent, which has not confined its operations to the coast, or to the periphery of the basaltic area. We can trace it, says the author, over its whole surface.

Some persons have maintained that the inequalities are those of original conformation; as if the world had come from the hands of the Creator with all the varieties which now contribute so much to its beauty.

Others, admitting the original continuity of the strata, and their subsequent abruption, have differed concerning the direction in which the cause has acted; some preferring the milder and more gradual operation of waters from above the surface, which, according to Dr. Richardson, rather tend to level than to raise inequalities; while others conceive the highest mountains to have been blown up from the bottom of the sea by furious explosions, which, in the author's

estimation, can never have taken place beneath the present rectilinear and parallel strata.

Dr. Richardson assumes as a fact, that strata having very strong resemblance were once continuous, however interrupted we now find them; for instance, the stratified remnants at the tops of the Seafin and Slievegallon, between which the valley of the Mayola is an excavation 1700 feet deep and three miles wide, were originally connected in their present position by similar materials, the whole of which have been completely carried away. And again, to the northward, between Seafin and Carntogher, the same stupendous operations have carried away the parts which formerly connected these undisturbed remains of the same strata.

The number of basaltic hummocks thus left on the tops of various mountains, is represented to be considerable, as if they had been left by the unknown sculptor for the express purpose of showing how high the original surface of the country formerly reached,—a conclusion which appears formidable; but the author does not admit that anything is absurd, incredible, or impossible, in geology and cosmogony.

A Letter on the Differences in the Structure of Calculi, which arise from their being formed in different Parts of the Urinary Passages; and on the Effects that are produced upon them, by the internal Use of solvent Medicines, from Mr. William Brande *to* Everard Home, *Esq. F.R.S.* Read May 19, 1808. [*Phil. Trans.* 1808, *p.* 223.]

Mr. Brande's observations were made during an examination of the calculi contained in the Hunterian Museum, and of some also in the possession of Mr. Home.

Three calculi, formed in the kidneys, were examined. One consisted of uric acid, nearly pure; another, weighing seven grains, contained $4\frac{1}{2}$ uric acid, and $2\frac{1}{2}$ animal matter. A third consisted of oxalate of lime; and it is added that a fine powder is also voided from the kidneys, consisting of the ammoniacal phosphate of magnesia, and of phosphate of lime.

Calculi retained in the infundibula, or pelvis of the kidneys, may be increased either by a deposition of uric acid, or may be coated by an external lamina, consisting of the phosphates.

Calculi met with in the bladder, are of four kinds.

1. Formed on nuclei of uric acid, from the kidneys.
2. On nuclei of oxalate of lime, from the kidneys.
3. Formed on sand or mucus deposited in the bladder.
4. Formed on extraneous bodies introduced into the bladder.

Those consisting of uric acid vary in colour, from a deep reddish brown to a pale yellowish brown. Those containing phosphate of lime, and the triple phosphate of magnesia, are whiter, and are often soft and friable. Those which contain oxalate of lime, called mulberry calculi, are browner, harder, and less soluble.

Out of 150 examined by Mr. Brande,

16 were composed of uric acid.

45 principally uric acid, with a small proportion of the phosphates.

66 principally phosphates, but containing a small quantity of uric acid.

12 phosphates only.

5 uric acid, and phosphates on a nucleus of oxalate of lime.

6 chiefly oxalate of lime.

It is observed by Mr. Brande, that calculi from the bladder, consisting of uric acid, contain a larger proportion of animal matter combined with it, than is usually found in kidney-calculi. One calculus, weighing twenty-five grains, being digested for two hours in water, lost 5½ grains, which were found, after evaporation of the water, to be principally urea, combined with some muriate of ammonia, and a little of the triple phosphate of magnesia.

Sixty grains of another calculus also yielded 5·2 of urea to alcohol; and being afterwards treated with acetic acid, they lost six more grains by solution of triple phosphate, and the remaining 48·8 were pure uric acid. It is observed, that the presence of the triple phosphate along with uric acid, would occasion all the phenomena that have been ascribed to a supposed urate of ammonia.

Of Calculi from other animals, examined by Mr. Brande.—One from the kidney of a horse contained 76 phosphate of lime, 22 carbonate of lime: another from the bladder of a horse, 45 phosphate of lime, 28 triple phosphate of magnesia, 10 carbonate of lime, and 15 animal matter.

Calculi from the bladder of an ox were found to be carbonate of lime with animal matter.

A calculus from a sheep's kidney contained 72 phosphate of lime, 20 carbonate of lime, and 8 animal matter.

The sediment from the urine of a rhinoceros was principally carbonate of lime, with a small proportion of phosphate of lime.

A large calculus, from the bladder of an old dog, contained 64 phosphate of lime, 30 ammoniacal phosphate of magnesia, and 6 of animal matter.

A calculus from a hog's bladder was principally carbonate of lime.

One from a rabbit contained phosphate of lime, and carbonate of lime, nearly in equal quantities.

Because of the difficulty of accounting for the formation of oxalate of lime, Mr. Brande has frequently examined the urine of calculous patients, but has never been able to detect it, either there or in healthy urine.

With respect to the use of solvents, it is observed, that those which have been recommended, under different circumstances, are of two kinds, acid or alkaline, opposite in their nature to each other, and accordingly each liable to occasion whatever the other is adapted to remove. The alkalies may prevent the formation of uric calculi, but will increase the disposition to form the phosphates. So also the acid, which dissolves the earthy phosphates, will at least have no action upon a nucleus of uric acid, and may occasion it to be increased by a fresh deposition.

Some Observations on Mr. Brande's *Paper on Calculi. By* Everard Home, *Esq. F.R.S.* Read May 19, 1808. [*Phil. Trans.* 1808, *p.* 244.]

In consequence of Mr. Brande's observations, that either acids or alkalies may be attended with injurious consequences, Mr. Home adduces various cases, for the purpose of doing away the expectation generally entertained, of relief from the use of solvents.

The first case is that of a person who had been relieved of the symptoms of calculus while taking saline draughts in the state of effervescence, but in whose bladder were found, after death, as many as twenty calculi; but the prostate gland had become enlarged, and had formed a barrier, so as to prevent the neck of the bladder from being irritated by them.

The second patient had used Perry's lixivium, with the same apparent benefit, which, in fact, arose from the same cause as the preceding.

Mr. Home has also found calculi in cysts, between the fasciculi of the muscular coat of the bladder, even so many as three or four in the same bladder, in which cases the usual symptoms of stone would not occur.

A gentleman having, at the age of seventy, voided a small uric calculus during a course of alkaline medicines, continued to use them at intervals for four or five years, suffering occasionally in a slight degree, but passing no more calculi. After his death about 350 light spongy calculi, consisting of the phosphates cemented by uric acid, were found in his bladder, which, in Mr. Home's estimation, were occasioned by the use of alkalies, in the manner suggested by Mr. Brande.

Another gentleman, who was found, by sounding, to have a stone in his bladder, took fossil alkali for about three months, after which he underwent the operation of lithotomy. The stone was found, externally, composed of pure triple phosphate of magnesia, in spiculated crystals, while the central parts had also a mixture of uric acid with the phosphates, so that the alkali had prevented the formation of uric acid; but the deposition of the phosphates appeared to Mr. Home more rapid than before.

On the Changes produced in Atmospheric Air, and Oxygen Gas, by Respiration. By W. Allen, *Esq. F.R.S. and* W. H. Pepys, *Esq. F.R.S.* Read June 16, 1808. [*Phil. Trans.* 1808, *p.* 249.]

The importance of a process so essential to life having excited proportional curiosity in philosophers from the earliest ages, the authors of the present communication take occasion to trace the history of their subject. Beginning with the conjectures of Hippocrates and of Plato, they proceed to notice the first accurate notions of Boyle and of Mayow, which were neglected and forgotten till the time when Priestley and Scheele first distinguished the two constituent parts of the atmosphere from each other.

The next discovery of importance on respiration, is that by Dr. Black, who observed the formation of carbonic acid. Succeeding labourers in the same field of inquiry, it is observed, are too numerous for justice to be done to every one; and the principal information collected from them relates to measures of quantity. Dr. Goodwin estimated the residual gas in the lungs, after expiration, at 109 inches. Dr. Menzies found the absorption of oxygen nearly 52,000 inches in twenty-four hours.

Lavoisier and Seguin, from a series of elaborate experiments, concluded that more oxygen was absorbed than evolved, as carbonic acid, and thence imagined that water was formed by the union of oxygen and hydrogen in the lungs.

Their experiments showed the consumption of oxygen to be greater in a colder atmosphere, and to be increased also during digestion and during exercise; and they estimated the average consumption at 41,000 inches per day.

The quantity of carbonic acid formed, was first estimated by Mr. Davy.

The authors conceiving that many important points are not yet satisfactorily settled, undertake to examine in the present communication,—

1. The average quantity of oxygen converted into carbonic acid in ordinary respiration.

2. Whether oxygen is absorbed by the blood.

3. Whether azote is absorbed, and whether hydrogen or other gas is evolved.

On account of the impossibility of knowing, with precision, the quantity of gas remaining in the lungs after expiration, and the consequent difficulty of deciding whether any gas is absorbed in the act of respiration, it was determined to perform the experiments on such large quantities of air at a time, that the error arising from residual gas might bear a small proportion to the whole quantity.

The apparatus for the first five experiments consisted of two gasometers, one of which contained 4200 inches of atmospheric air over water, from which the inspirations were made, and the other being filled with mercury, was employed to receive the gas after expiration; but as its capacity was only 300 inches, it was necessary for the operator to retain his breath for about fifteen or twenty seconds, till the quantity expired had been read off, noted, and expelled from the gasometer; after which the same process was repeated about twelve times. The quantity of inspired air having now been read off from the water gasometer, the quantity expired was ascertained by casting up the amount of the successive fillings of the mercurial gasometer. From an average of these five experiments, there appeared to be a diminution of twenty inches in 3700. But the authors are inclined to ascribe this difference to the difficulty above mentioned, of bringing the lungs to the same state of contraction after the experiment as they had been before.

In determining the *quality* of the expired gas, lime-water was first

employed for the absorption of carbonic acid, and then a solution of green sulphate of iron, saturated with nitrous gas, to determine the quantity of oxygen that remained.

In the sixth experiment there were found 9 carbonic acid, and 9 oxygen; together 18.

In the seventh experiment there were found 8 carbonic acid, 10½ oxygen; together 18½.

In the eighth experiment there were found 6½ carbonic acid, and 12½ oxygen; together 19.

In the ninth experiment there were found 7 carbonic acid, and 11 oxygen; together 18.

So that as one measure of carbonic acid contains just an equal measure of oxygen, the apparent diminution of oxygen in these experiments is 2½ per cent.

In the subsequent experiments two mercurial gasometers were employed, so that the hurry of measuring the gas, and inconvenience of retaining the breath, were avoided, and the respiration was consequently more natural throughout the whole of each experiment.

Nevertheless, the result of the ten experiments still showed an apparent diminution of 1 per cent. But the eleventh experiment showed no diminution, and is considered by the authors as a standard experiment. From this the quantity of oxygen consumed, and of carbonic acid formed in a minute, is calculated to be 26½ inches, and hence the quantity of solid carbon given off by the lungs, in twenty-four hours, is computed at 10¾ ounces troy.

The person who made these experiments breathes about nineteen times in a minute, and takes about 16½ cubic inches of air at each natural inspiration.

In the twelfth experiment the same quantity of air was breathed from one of the mercurial gasometers to the other repeatedly during three minutes, and was then found to contain 9½ carbonic acid, 5½ oxygen, and 85 azote per cent.; so that six parts, in 100 of oxygen, seemed to have disappeared, and some other gas, not absorbed by water, was apparently given off from the lungs.

The results of the fourteenth experiment were similar, and more remarkable, as the respiration of the same air was continued for a greater length of time: 100 parts contained 10 carbonic acid, 4 oxygen, and 86 azote; so that in this instance there appeared a loss of 7 oxygen, and an equal increase of azote, or of some other gas not absorbable by water. By careful examination, the authors are satisfied that no other gas is present in the residuum but azote; that the azote itself, though apparently in greater proportion, is in fact unaltered; and that the difference arises solely from diminution of oxygen when respiration is painfully protracted.

By the respiration of another person, whose chest was of greater capacity, the same quantity of air as by the former was taken in just one half the time; but nevertheless very nearly the same proportion of it was found converted into carbonic acid.

In the succeeding experiment nearly 10,000 cubic inches of air

were breathed. The quantity of carbonic acid was in this case 8, the remaining oxygen 13, and the azote 79 per cent., as before inspiration. The average consumption of oxygen, and formation of carbonic acid, being 32·3 inches in a minute.

The deficiency in the quantity expired was so small, that the authors were satisfied that none was absorbed. But in the two following experiments, which were each performed on upwards of 3000 inches of *oxygen* gas, the deficiency was much greater, and there *did* appear to have been absorption of 58 in one experiment, and of 67 in the other.

The formation of carbonic acid was, in these cases, far more rapid, being at the rate of 37½ inches in a minute. From the quantity of air which had necessarily remained in the lungs before the experiments, a quantity of azote was unavoidably mixed with the inspired oxygen, and emitted with it, in the proportion at first of 25 per cent., but varying in the successive portions, removed for trial, till the proportion was at last reduced to 5½ per cent.

From these data, the authors endeavour to estimate the quantity of air that had been in the lungs at the beginning of each experiment.

By estimation from the former experiment on oxygen, the residue in the lungs appeared to have been 140½; but in the second, it was found that as much as 177 of mere azote had been expired, and hence that the lungs, in this instance, had contained as much as 226 inches of air, unless the proportion of azote in the residual gas was greater than usual, in consequence of previous fatigue.

The inferences drawn by the authors from these experiments are,

1. That the quantity of carbonic acid emitted is equal in bulk to the oxygen consumed, and consequently that no water is formed in respiration by union of oxygen and hydrogen.
2. That carbonic acid, in expired air, varies from 8 to 10 per cent.
3. That about 1800 inches of carbonic acid are formed in twenty-four hours, containing about 10¾ ounces troy of solid carbon.
4. That when air is breathed several times over, a portion of oxygen is absorbed.
5. More carbonic acid is formed from respiration of oxygen, than in breathing atmospheric air.
6. That the subject of these experiments takes about 17 inches at each inspiration, and makes about 19 inspirations in a minute.
7. No hydrogen or other gas appears to be evolved during respiration.
8. There appears to be no alteration in the quantity of azote, since the average diminution of six parts in 1000 is more likely to arise from incomplete exhaustion of the lungs after the experiment.
9. The residual gas in the lungs, after forced expiration, appears about 140 cubic inches.

Description of an Apparatus for the Analysis of the Compound Inflammable Gases by slow Combustion; with Experiments on the Gas from Coal, explaining its Application. By William Henry, *M.D. Vice-Pres. of the Lit. and Phil. Society, and Physician to the Infirmary, at Manchester. Communicated by* Humphry Davy, *Esq. Sec. R.S.* Read June 23, 1808. [*Phil. Trans.* 1808, *p.* 282.]

The object of this memoir is to remove some obstacles which present themselves to a successful inquiry into the nature of compound inflammable gases. When a vegetable substance is submitted to distillation, at a temperature not below that of ignition, the equilibrium of affinities is destroyed, and the elements composing it are united in a new manner. The carbon, uniting with oxygen, either composes carbonic acid gas, or stopping short of that degree of oxygenation, is converted into carbonic oxide. The hydrogen, combining with a portion of charcoal, forms either carburetted hydrogen gas, or super-carburetted hydrogen, better known by the name of olefiant gas. Towards the close of the process, simple hydrogen gas is also mingled with the products.

The method hitherto pursued for the analysis of inflammable gases, has consisted in mixing them with a known proportion of oxygen, burning the mixture by the electric spark, noting the first contraction and subsequent diminution, by lime-water, for the quantities of water and of carbonic acid formed, and thence inferring the proportion of hydrogen and of carbon in the gas examined, without making due allowance for the possible admixture of carbonic oxide, either before or after the combustion.

The author ascertains the presence of this gas by its great specific gravity, and by the very small proportion of oxygen with which it unites; but in the distillation of coal there is also often present a quantity of sulphuretted hydrogen, and an inflammable gas, which may possibly be a gas *sui generis*, but is more probably a mixture of carburetted hydrogen and carbonic oxide.

In Dr. Henry's apparatus, which is not readily explained without reference to a drawing, the gas to be examined is passed through a small tube into a receiver containing oxygen gas, and is there burned by a continued flame. The quantity of gas introduced, and quantity of contraction, are noted. The quantity of carbonic acid formed is found by liquid potash. The remaining oxygen is determined by sulphuret of lime. As it sometimes happens that part of the gas escapes combustion, precautions are given for avoiding that source of error, and the means of detecting it described.

Instances are given of computation, from combustion, of hydrogen gas, of olefiant gas, as the most simple, and of gas from cannel coal, in which it is a problem of some difficulty to determine the proportions of all the gases that occur; and these are carbonic acid, sulphuretted hydrogen, olefiant gas, carburetted hydrogen, and carbonic oxide.

The sum of the two first is determined by liquid potash, after

which the third is ascertained by oxymuriatic gas. The sum of the second and third may be determined by oxymuriatic gas, and then the first absorbed by liquid potash. The first and third being thus known, the second may be inferred, though not removed alone.

The nature of the inflammable residuum is then learned by combustion, due regard being had to its specific gravity, and to the proportion which the carbonic acid formed bears to the oxygen consumed.

A table is given of the products of various gases, from several different species of coal; and a second table, of the specific gravities of these gases, with the quantities of carbonic acid formed from each, and the quantity of oxygen required for their combustion. It is observed generally, that in the distillation of all these substances, the gas received at the commencement of the process is the heaviest, that it requires the largest proportion of oxygen, and is fittest for the purpose of illumination.

An Account of some Peculiarities in the anatomical Structure of the Wombat, with Observations on the female Organs of Generation. By Everard Home, *Esq. F.R.S.* Read June 23, 1808. [*Phil. Trans.* 1808, *p.* 304.]

The author, having received a male wombat alive from one of the islands in Bass's Straits, had an opportunity of observing its habits in a domesticated state, and of examining the peculiarities of its internal formation after death, particularly the mechanism of the bones and muscles of its hind legs, which have not been described either by Geoffroy, in his account of its internal form, or by Cuvier, who has described several parts of its internal structure in his *Leçons d'Anatomie Comparée*. The stomach of the wombat resembles closely that of the beaver, and differs so much from that of the kanguroo, and other animals of the opossum tribe, that it forms an extraordinary peculiarity. An account of the dissection of a female wombat having been received from the late Mr. Bell, Surgeon to the Colony at New South Wales, Mr. Home has inserted Mr. Bell's description, with remarks especially on the state of the uterus, which was double, and impregnated on each side; that on the right side was as large as a pullet's egg. The os tincæ was filled with a thick gelatinous substance. When a longitudinal incision was made into its cavity, its coats were found lined with the same jelly, in the centre of which was an embryo wrapped up in very fine membranes, that appeared to have no connexion by vessels with either the uterus or the gelatinous matter.

These facts, says Mr. Home, throw considerable light on the mode of propagation of this very curious tribe of animals. They confirm, in the most satisfactory manner, the observations contained in a former paper on the kanguroo, which required further evidence, as the specimen on which the observations were made had been sent to England preserved in spirits, and the parts had become very indistinct, from being coagulated and long kept.

Since it seems thus established, that the fœtus is nourished without a placenta, the source of the gelatinous matter provided for its nourishment becomes a natural object of inquiry. In birds the albumen of the egg, which corresponds with this jelly, is formed in a tube called the oviduct, and the lateral canals, which open into the cavity of the uterus of the wombat and kanguroo near its neck, bear a strong resemblance in their form to the oviducts of birds; and in the kanguroo they were found, in the impregnated state, to be enlarged, and to have a very free communication with the uterus; circumstances which induce the author to believe their sole use is to form the jelly, and to deposit it in the uterus.

On the Origin and Office of the Alburnum of Trees. In a Letter from Thomas Andrew Knight, *Esq. F.R.S. to the Right Hon. Sir* Joseph Banks, *Bart. K.B. P.R.S.* Read June 30, 1808. [*Phil. Trans.* 1808, *p.* 313.]

Mr. Knight having, by his former experiments on this subject, established to his satisfaction that the bark of trees is not converted into alburnum, but that the alburnous matter is deposited by a fluid which descends from the leaves, and is subsequently secreted through the bark, proceeds to inquire into the origin and office of the alburnous tubes.

In the succulent shoot of the horse-chestnut it may be seen, that the alburnous tubes are arranged in ridges beneath the cortical vessels, and the number of these ridges at the base of each leaf corresponds with the number of apertures which pass from the leaf-stalk. The position and direction of these tubes have induced naturalists to consider them as passages through which the sap ascends; but Mr. Knight intends to show that they are reservoirs of fluid secreted by the bark, which they retain till it is absorbed by the surrounding cellular substance. Having ascertained, by injections, that the alburnous tubes which descend from the base of an annual shoot are confined to one side of the stem, and to the external annual layer of wood, Mr. Knight made deep incisions at the bases of many annual shoots of young trees, in such a manner as certainly to intercept all communication with the stem by means of the alburnous tubes; but the shoots lived, and in the succeeding spring grew with considerable vigour. In some small twigs, two incisions were made on opposite sides, one a few lines higher than the other, so that all the alburnous tubes might, in some part of their course, be interrupted; yet the sap continued to pass into the branches, and their buds unfolded in the succeeding spring.

All naturalists agree in stating that trees perspire most during summer, when the leaves have attained their full growth; but as the alburnous tubes at that time appear dry and filled with air, it is evident that the tubes do not convey the sap, but are intended to execute a different office.

But if the sap does not rise through the alburnous tubes, says

Mr. Knight, it must pass the cellular substance; and to show that the cellular substance is pervious to fluids, he quotes an instance from a preceding communication, in which bark was generated on the surface of the cellular substance of pollard oaks. A new experiment is also related in confirmation of this opinion. Various branches of two years old were cut from different trees; and after the cut surface had been covered with a cement impermeable to water, a portion of the bark was detached from each, so as to expose the surface of the alburnum. They were then immersed in coloured infusions for twenty hours, and were found to have imbibed the colour between the alburnous tubes, apparently through the cellular substance.

When an incision is made into the alburnum near the root, and sap is seen to flow from it, it may be observed to run even more abundantly from the upper surface of the incision; and since the quantity which exudes is full a hundred times more than was previously contained in the alburnous tubes, it appears evident that it is raised through some other channel.

Mr. Knight concludes, therefore, that the sap ascends through the *cellular* substance, and conceives that part to be so organized as to permit the sap to escape more readily *upwards* than in any other direction; and, by its alternate contraction and expansion, to be fully capable of propelling the sap with all the impulse which it is known to have in the spring.

The alburnous tubes appear to the author to answer also another purpose, since their cylindrical form occasions the strength of the materials employed to be the greatest possible.

The author concludes by observing, that though he retracts an opinion formerly entertained respecting the ascent of sap through the alburnous tubes, yet his opinions concerning its subsequent motion through the central vessels, leaves, and bark, remain unchanged.

Eclipses of the Satellites of Jupiter, observed by John Goldingham, *Esq. F.R.S. and under his Superintendance, at Madras, in the East Indies.* Read June 30, 1808. [*Phil. Trans.* 1808, *p.* 322.]

Mr. Goldingham's observations are prefaced by a short account of the instruments employed, and some general remarks upon the circumstances necessary to be attended to, in drawing inferences from them. Two telescopes were employed, both made by Dollond, of 3½ feet focal length, with a magnifying power between 70 and 80; and the time observed by a good clock, with gridiron pendulum, was deduced from the transit of the sun nearest to the eclipse, and verified by the next preceding or following transit.

The longitude of the place of observation is given, as determined from numerous observations of various kinds, that it may be compared with that deduced from each eclipse by means of the time given in the ephemeris.

In noting each observation, the state of the weather is attended to,

the altitude of the planet, the relative situation of the planet and moon, with any other circumstances of twilight or haze that were likely to cause any incorrectness in the observation. To obviate the uncertainty that will always attend the results of such observations, from different states of weather, difference of telescopes, and imperfection of vision in different observers, Mr. Goldingham recommends not to draw any inference from comparison of corresponding immersions alone, without attending to the difference of corresponding emersions also, by which all sources of error will be in a great degree corrected.

Electro-Chemical Researches on the Decomposition of the Earths; with Observations on the Metals obtained from the alkaline Earths, and on the Amalgam procured from Ammonia. By Humphry Davy, *Esq. Sec.R.S. M.R.I.A.* Read June 30, 1808. [*Phil. Trans.* 1808, *p.* 333.]

In the last Bakerian lecture mention was made of an apparent decomposition of barytes and strontites into oxygen and inflammable matter.

In the prosecution of the same course of experiments upon lime and magnesia, similar results were obtained; for when either of these earths was slightly moistened with water, and thereby rendered a conductor of electricity from a powerful voltaic battery, inflammable matter was developed at the negative surface, and oxygen at the positive. And these phenomena were not dependent on the presence of water; for when the same earths were made conductors, by being in a state of fusion with boracic acid, analogous appearances were produced.

On account of the high inflammability of these bodies, similar experiments were conducted under naphtha, with the hope of preserving the products for examination; but although dark opake specks, having in some cases an appearance of metallic splendour, were produced, yet, when heat was applied to them under boiling naphtha, there was no appearance of fusion, and consequently no separation from the surrounding undecompounded earth; but when the mass was afterwards thrown into water, an effervescence occurred, and evident decomposition of the water.

A series of experiments were next undertaken upon mixtures of barytes or strontites, of lime or magnesia, of alumine or silex, with potash; and in the case of barytes and strontites, the potassium formed was evidently not pure, but apparently alloyed by another metallic substance.

When barytes, strontites, or lime, was mixed with oxide of silver or mercury, the compounds afforded analogous results when acted on by the voltaic battery.

When these experiments were resumed, after an interval of several weeks, a battery had been prepared with 520 pair of plates; and an attempt was then made to unite the bases of these earths with the

wire by which the power was conveyed from the negative end of the battery. Lead, silver, copper, and iron, were successively employed; and it was by means of the last that the most distinct results were obtained.

When an iron wire, one seventieth of an inch in diameter, was made the conductor on the negative side, and brought into contact with moistened barytes or strontites, lime or magnesia, alumine or silex, the globule formed by fusion at the end of the wire appeared in each case to be alloyed with something capable of acting upon water; but those last named had, in succession, less action than the preceding.

The author had not himself used mercury as a means of obtaining the bases in a state of alloy till informed by a letter from Professor Berzelius of Stockholm, who has used it with success for the decomposition of lime and barytes. And Mr. Davy has found it to succeed equally with strontites and magnesia. And although mercury alone failed of effecting the decomposition of alumine and silex, yet when an alloy of mercury and potassium was made the medium of communication even with these bodies, they each appeared to be decomposed by assistance of the affinity of potassium for their bases.

The author also informs us of his success in repeating an experiment of Professor Berzelius and Mr. Pontin, on the decomposition of ammonia: a globule of mercury being inserted in a small cavity made in a piece of carbonate or muriate of ammonia slightly moistened, they are placed together on a plate of platina positively electrified, and the wire from the negative end of the battery is applied to the mercury. The globule soon increases very considerably in bulk, and becomes converted into a soft amalgam, which absorbs oxygen from the atmosphere, or decomposes water into which it is thrown, and forms ammonia, while the globule gradually recovers its fluidity, and is reduced to its original size before the experiment.

The Croonian *Lecture. On the Functions of the Heart and Arteries. By* Thomas Young, *M.D. For. Sec. R.S.* Read November 10, 1808. [*Phil. Trans.* 1809, *p.* 1.]

Since the degree and manner in which the circulation of the blood depends upon the muscular and elastic powers of the heart and arteries are questions belonging to the most refined departments of hydraulics, the author has already submitted to the Society those general principles upon which he designs, in the present lecture, 1st, to inquire what would be the nature of the circulation if the vessels were as inelastic as glass or bone; 2ndly, in what manner the pulse would be transmitted if the tubes were merely elastic; 3rdly, what actions may be ascribed to their muscular coats; and, lastly, what disturbances are occasioned in different kinds of fevers and inflammations.

In order to determine the velocity of the blood in different parts, it is necessary to estimate the pressure by which it is urged forward, and the resistance opposed to its motion. From the experiments of

Hales, the *pressure* may be considered as equivalent to a column of seven feet. In order to calculate the *resistance*, the author employs the theorems contained in his former communication, and adopts the measurements of Keill and others, for the diameters of the aorta, and of its successive subdivisions. The quantity of blood in the arteries is estimated at nine or ten pounds; its velocity in the aorta about eight inches and a half in a second; while that in the capillary arteries is about $\frac{1}{33}$rd of an inch in a second (the diameter of these vessels being about $\frac{1}{1100}$th of an inch). The resistance that would be opposed to *water* circulating under the same circumstances is calculated to be equivalent to a pressure of a column of twenty inches; but the resistance to the motion of the *blood* is supposed in consequence of its viscidity to be about four times as great, and is consequently stated as eighty inches.

The effects of curvature in increasing the resistance are not neglected, but they form a very small part, in comparison to the difference that would arise from assuming different dimensions for the arterial system; or different allowance for the resistance of vessels that are too small for direct experiment, or a different ratio for the assumed effect of viscidity.

The author next examines the nature and velocity of the propagation of the pulse, which he considers analogous to the motion and waves on the surface of water, or of sound transmitted through the air; the elasticity of the arteries being, in this case, substituted for the elasticity of the fluid. Since the blood in the human arteries is subjected to a pressure, which is measured by a column of about seven feet and a half, the velocity with which the impulse is transmitted will be about fifteen feet and a half in a second; but as the progressive motion of the blood itself is about eight inches in the same time, the aggregate velocity of a pulsation is considered as sixteen feet in a second.

The greatest velocity of the blood during the contraction of the heart being about one eighth part of that quantity, the area of the artery must be proportionally dilated during its passage, and the diameter must increase in the ratio of fifteen to sixteen.

The force of the heart necessary to occasion this distension, must be proportionally greater than the average, and must be equal to a column of 101 inches, which agrees extremely well with an experiment of Hales, on the ascent of blood in a tube connected with the artery of a horse.

The author acknowledges, however, that though the calculations agree perfectly with each other, and with experiments on the power of the heart, and affections of the smaller arteries, yet they do not correspond with the apparent alteration in the diameter of an artery exposed to view; and he infers that the velocity of the pulse in the larger arteries, is really much more considerable than has been stated.

With respect to the functions of the muscular fibres of the arteries, Dr. Young apprehends that it will appear demonstrable that they are much less concerned in the motion of the blood than is almost uni-

versally believed; and though a mola has sometimes been found in the uterus totally destitute of heart, and in which the blood must have circulated, in its usual course, through the arteries and veins, it cannot be known that there was any alternate pulsation; and, even if there was any pulsation, it must have arisen from preternatural power in the arteries, and the resistance in the extreme vessels must have been preternaturally small.

The service which the author imagines is performed by the muscular coats of arteries, is that of adapting their capacities to the varying quantity of the blood contained in them.

According to this statement, the qualities of the pulse, as discernible to the touch, are ascribed almost entirely to the action of the heart.

The duration from the natural state may consist either in a change of the heart or of the capacity of the smaller arteries. The quantity of blood transmitted may also remain the same, or be diminished, or be increased. When it remains the same, the change cannot be very material, and is not likely to lead to any disease of the vital functions. When the quantity transmitted is smaller than in health, the arteries must become contracted, and the veins distended. The pulse must be small and weak, until the blood accumulated in the veins stimulates the heart to greater action. From the vigour acquired during the former remission of its exertion, the pulse becomes full and strong, a greater quantity of blood being transmitted than in health, as seems to occur in the hot fit of fevers.

A relaxation of the extreme arteries may, at the same time, suffer the blood to pass more easily into the veins; but such a relaxation may be carried to excess, and the arteries be thereby emptied; so that the pulse becomes small and weak, and the heart exhausted by fruitless efforts to restore the equilibrium, as appears to occur in typhus.

As *general* derangements of the circulation appear to be concerned in different kinds of fever, so *partial* ones have a similar relation to local inflammations. The most obvious changes arise from partial dilatations or contractions of the capillary arteries. Distension will be accompanied with redness, pain, and heat. But it is possible that another species of inflammation may also arise from an *obstruction* of the extreme capillary arteries; so that the small branches are subjected to a pressure many times greater than that which they are intended to withstand in their natural state, and the consequent distension will be accompanied with redness and pain.

Besides these general illustrations of the nature of fevers and inflammations, the author is of opinion that the same theory may be of use in explaining the operation of remedies employed for relieving them, more especially (in the different modes of letting blood), the more speedy and effectual relief from opening an artery than a vein, and the great benefit often derived from the more practicable expedient of withdrawing a small quantity of blood from the immediate neighbourhood of the part affected, by cupping or by leeches.

An Account of some Experiments, performed with a View to ascertain the most advantageous Method of constructing a Voltaic Apparatus, for the Purposes of Chemical Research. *By* John George Children, *Esq. F.R.S.* Read November 24, 1808. [*Phil. Trans.* 1809, *p.* 32.]

The object of the author is to determine how the greatest effect may be produced by the voltaic battery, with the least waste of power and expense.

For this purpose he had one battery constructed of twenty pairs of plates of zinc and copper of large dimensions, each plate being four feet long and two feet wide. Each pair was connected together only at the top by a strap of lead, so that both sides of each plate were exposed to the action of the fluid in the trough. The trough was made entirely of wood, with wooden partitions, made water-tight by cement; and this battery when in action was charged with a mixture of three parts nitrous acid, with one of sulphuric diluted with three parts of water.

With this battery,

1. Eighteen inches of platina wire $\frac{1}{30}$ of an inch in diameter were fused in about twenty seconds.
2. Three feet of the same wire became visibly red by strong daylight.
3. Four feet of the same became very hot, but not visibly red.
4. Charcoal burned with intense brilliancy.
5. On iron wire, the effect was remarkably feeble. Not more than ten inches of the finest harpsichord wire could be fused by it.
6. Imperfect conductors were scarcely affected by it. No effect was produced upon barytes mixed with red oxide of mercury and water.
7. A gold-leaf electrometer was not affected by it.
8. The shock from this battery was scarcely perceptible.

The author's second battery consisted of 200 plates, about two inches square, placed in half-pint pots of common Queen's ware.

1. With this battery potash and barytes were readily decomposed.
2. The metallization of ammonia took place with great rapidity.
3. It visibly ignited charcoal.
4. It caused a strong divergence of the gold-leaf electrometer.
5. It gave vivid sparks for upwards of three hours, and was not exhausted till after forty hours.

The results of the foregoing experiments are considered as a confirmation of Mr. Davy's observation, that intensity increases with the *number*, and the *quantity* of electricity with the *extent* of the surface.

The effect of quantity is seen in the first experiment on platina wire. This metal not being oxidated presents no obstacle to the passage of the electricities, which evolve, on their mutual annihilation, heat sufficient to raise the temperature of the platina to the point of fusion.

Nevertheless from want of *intensity*, this quantity could not find a ready passage through the suboxidated iron wire, and could produce no effect upon barytes or other bodies liable to be decomposed by

the greater energy of the small battery. In this the number of plates being tenfold gives tenfold intensity, although the aggregate quantity of surface in the whole battery is not $\frac{1}{20}$th part of the acting surfaces in the large battery.

The advantage of a large quantity of fluid is evinced by the long-continued action of the small battery; and it is also observed that in very numerous combinations, a certain distance between the plates becomes necessary to prevent spontaneous discharges, which the author found to take place in a battery of 1250 plates of four inches square.

With this battery of 1250, excited by a fluid of the same strength as was used in the former experiments, the author ascertained the striking distance through the air to be $\frac{1}{50}$th of an inch, care having been taken to dry the air, through which the discharge took place, before the experiment, as well as to avoid any increase of temperature previous to the discharge.

The electric *light* was also made to pass through a vacuum, and was observed to be the same as from a common electrical machine.

The effect of this great number of plates on imperfect conductors, was of course uncommonly powerful, but yet their *power of fusion* was comparatively weak, as they barely melted half an inch of the same platina wire that had been used in the former experiments; and hence it is evident that the construction must be different according to the purpose for which the battery is designed.

For igniting perfect conductors large plates are necessary, but they need not be numerous; and for overcoming the resistance of imperfect conductors number is requisite, but the size of the plates may be small.

The new method of constructing the trough wholly of wood, with moveable plates joined together only at top, is much preferred to the old construction, as the plates are more easily cleaned or repaired, and as they expose double extent of surface.

The Bakerian *Lecture. An Account of some new analytical Researches on the Nature of certain Bodies, particularly the Alkalies, Phosphorus, Sulphur, Carbonaceous Matter, and the Acids hitherto undecompounded; with some general Observations on Chemical Theory. By* Humphry Davy, *Esq. Sec. R.S. F.R.S. Ed. and M.R.I.A.* Read December 15, 1808. [*Phil. Trans.* 1809, *p.* 39.]

The objects which principally occupied Mr. Davy's attention in the present lecture are, the elements of ammonia; the nature of sulphur; the nature of phosphorus; the states of the carbonaceous principle in plumbago, charcoal, and diamond; the analysis of boracic acid; the analysis of fluoric acid; with a series of numerous experiments on muriatic acid.

With respect to ammonia, he has been induced to reconsider the subject, not from any doubt which he himself entertained of the correctness of his former results, but on account of the opinion still

maintained by Messrs. Berthollet on the non-existence of oxygen in ammonia, and on account of the inference deduced by Messrs. Gay-Lussac and Thenard, from the action of potassium on ammonia, who conclude that potassium is a compound of potash and hydrogen. Mr. Davy consequently details a variety of processes in which he examined all the circumstances and results of their mutual action.

The potassium employed was procured by passing dry potash through red-hot iron turnings in a gun-barrel, which appears to differ from that obtained by electricity solely in containing a very small portion of iron.

A green glass retort, after the introduction of a piece of potassium, is first exhausted, then filled with ammoniacal gas, and after two subsequent exhaustions, finally filled again with the gas in an extremely pure state. The potassium thus exposed to ammonia at common temperatures, loses its lustre and becomes white by a thin crust of potash on its surface; while the gas suffers a slight diminution, and then contains about $\frac{1}{10}$th of its bulk of hydrogen.

When the potassium is heated by a spirit-lamp, the colour changes from white to a bright azure, thence to bright blue, green, and olive, which is the last state to which the whole of the potassium may be brought by continuation of the heat. When sufficient ammonia is present to insure the complete saturation of eight grains of potassium, twelve cubic inches of ammoniacal gas disappear, and nearly eight cubic inches of hydrogen are evolved. The French chemist having stated this quantity to be exactly equal to that given out by the action of an equal quantity of potassium on water, Mr. Davy has made the comparison with great care, and finds the quantity of hydrogen given out by its action upon water to be just $8\frac{1}{2}$ cubic inches.

The olive-coloured compound formed is combustible, heavier than water, and a conductor of electricity. It fuses at a low temperature, and then begins to emit ammonia, till its quantity amounts to $4\frac{3}{4}$ inches out of 12 cubic inches that had disappeared. The residuum is then no longer fluid, and begins to give off hydrogen and nitrogen, till the former amounts to 4 inches and the latter to $1\frac{4}{10}$, in proportion exactly suited to the formation of ammonia. When this residuum no longer yields any gas, even at a red heat, a quantity of ammonia may still be formed from it by the addition of water, and amounting to about four cubical inches, and along with these about $\frac{3}{10}$th of an inch of hydrogen. The formation of *ammonia* in this case having proved that a quantity of nitrogen was here combined with the potassium, other experiments were instituted for the purpose of obtaining it separate. By combustion in oxygen gas, a part of the nitrogen, but not the whole, was obtained; but by distillation with red oxide of mercury, the product of nitrogen was greater. For the formation of *ammonia* from this nitrogen, as much hydrogen is wanted as was originally given out by the ammonia, in the first part of the action of potassium upon it; but unless *oxygen*, as well as hydrogen, be sup-

plied, neither potash nor ammonia can be produced; and if oxygen merely be applied, potash and nitrogen are the result.

In consequence of the supposition of Messrs. Gay-Lussac and Thenard, that they had formed a compound of potassium and hydrogen, Mr. Davy repeated their experiment frequently, without any success; neither has he, by any other means, been able to form a compound of hydrogen with potassium.

In the residuum obtained by heat, after the action of potassium on ammonia, the nitrogen appears to be combined with an oxide of potassium, in which the oxygen amounts to about three per cent. By greater heat this compound itself sublimes, and does not yield nitrogen without the intervention of oxygen; as if some portion of the latter were essential to the constitution of nitrogen gas.

Mr. Davy refers to an hypothesis formerly advanced, that all metals may possibly be compounds of unknown bases with hydrogen; but replies to those arguments by which Messrs. Gay-Lussac and Thenard imagined that they had proved the existence of hydrogen in potassium; for which there appears to be no foundation in fact. Until hydrogen can be separated from some metallic substance,—until a metal can be deprived of its imflammability by the separation of hydrogen, that theory must be preferred, which, in explaining all the facts, admits the presence of no ponderable agents of which the existence cannot be proved.

Mr. Davy next proceeds to an examination of sulphur, first by passing discharges through it in a fluid state from a common electric machine, but afterwards with better success by the voltaic battery, and obtained gas from it in sufficient quantity to ascertain that the gas consists wholly of sulphuretted hydrogen. In the course of the process, the sulphur had acquired the power of reddening litmus. After long-continued electrization, the sulphur became extremely difficult of fusion and acquired a dirty brown colour.

By the action of potassium on sulphur, sulphuretted hydrogen is also evolved with intense heat and light; and the circumstances of this operation appear to be similar to what occurs when potassium is heated in contact with resin, camphor, wax, and fixed oils, in close vessels. For in this case also, great heat is generated, and great quantities of carburetted hydrogen evolved. In addition to this analogy in their chemical actions, Mr. Davy also remarks, that the physical qualities of these bodies resemble those of sulphur.

They agree in being non-conductors, whether fluid or solid; transparent when fluid, but semi-transparent when solid, and highly refractive; but resinous and oily bodies contain a small quantity of hydrogen and oxygen, with a large quantity of carbonaceous matter. So also in sulphur, the mixture of hydrogen is fully proved, and the existence of oxygen might be inferred from the effect of the residual sulphur on litmus paper, but is more distinctly evinced by the formation of potash when potassium is heated in sulphuretted hydrogen. From such experiments as were most to be depended upon, it

is inferred that the quantity of oxygen in sulphur amounts to about ten per cent., and hence the intense ignition that occurs in the union of sulphur with potassium, and other metallic bodies, is traced to a more probable source than their mere affinity for sulphur.

The same analogies apply to phosphorus as to sulphur; the same mode of operating was adopted, and products perfectly analogous were obtained. By electrization, phosphuretted hydrogen was given out, and the phosphorus became of a deep red brown colour. By the action of potassium also, phosphuretted hydrogen was obtained, and by the action of acids on the residuum, it appeared that the potassium had gained oxygen from the phosphorus; and the same inference is drawn from the action of potassium on phosphuretted hydrogen, which appears to contain oxygen in a state of combination, similar to that which obtains in sulphuretted hydrogen.

The same new modes of research are next employed to discover what differences subsist in the states of carbonaceous matter, in plumbago, charcoal, and diamond; for though late and very accurate experiments have proved that they yield very nearly the same quantities of carbonic acid, it was nevertheless not improbable that new means of analysis might detect chemical differences, correspondent to the extreme difference of their physical properties.

Plumbago, whether acted upon by the voltaic battery or by potassium, yields no elastic product in either case; but in the latter, merely combines with the potassium unaltered. Charcoal, on the contrary, by the voltaic apparatus, yielded a considerable quantity of carburetted hydrogen, but did not contribute to the oxidation of the potassium, any more than plumbago had done.

The unconducting nature of the diamond rendered it impossible to apply the voltaic battery with any effect, but it was by no means insensible to the action of potassium. When these substances were heated together, there was no intensity of action, and no production of elastic fluid. But the diamond soon blackened at its surface, and was ultimately reduced to a state perfectly resembling plumbago; part of it at the same time uniting to the potassium. The addition of carbonaceous matter was not, however, the sole change that had happened to the potassium, as it now extricated a smaller quantity of hydrogen from water than an equal quantity of pure potassium, and had evidently acquired a portion of oxygen from the diamond. This quantity of oxygen (though certainly very small) is thought to be the cause of its non-conducting quality.

Mr. Davy next resumes the analysis of boracic acid, which he had begun in his last Bakerian Lecture. By means of voltaic electricity, a black matter could be obtained from it that was unaltered by water, but soluble in nitric acid, and when heated to redness, burned slowly, giving off white fumes. But the quantity of the base that could be thus obtained was too minute for determining distinctly its relation to the acid from which it was produced. However, by heating together boracic acid with potassium, a large quantity of a similar matter (as has also been observed by M. Thenard) was obtained. In

this experiment intense ignition took place at the point of contact of the substances, the potassium appearing to burn by oxygen acquired from the acid, of which eight grains saturated about twenty of potassium.

The residuum did not effervesce in water, which merely dissolved some sub-borate of potash which is formed, and leaves exposed the boracic base as a spongy mass, black in some parts, and dark olive in others. It appeared to be infusible by heat, but a perfect conductor of electricity. When acted upon by nitric acid, or burned in oxygen, it was reduced again to the state of boracic acid, probably much heavier than the basis from which it is formed.

When fluoric acid gas was acted on by potassium, fourteen cubic inches disappeared by means of ten grains and a half of potassium, and about two inches and a quarter of hydrogen gas were evolved, apparently from water contained in the gas. In this experiment, a brownish sublimate was sometimes raised by the heat generated, and at others, a blackish matter remained mixed with a quantity of fluate of potash that is formed.

This matter appears to be fluoric acid, deprived of oxygen, and existing in a state analogous to that of sulphur and phosphorus; for when the sulphuric or phosphoric acids are decomposed by potassium, the pure bases are not evolved, but sulphurets and sulphites, phosphorets and phosphites, are generated.

Although the attempts to decompose the muriatic acid have not hitherto been equally successful with the preceding, yet many new and interesting results were obtained. When a quantity of potassium was employed, sufficient to absorb a given quantity of this gas, so much hydrogen was evolved as to prove that it contains full one third its weight of water.

Various attempts were made in consequence, to obtain the acid free from water, but they only terminated in new and singular combinations.

By burning phosphorus in oxymuriatic acid, a very volatile compound was obtained, consisting, apparently, of muriatic acid and phosphoric acid in a dry state, and a second compound of phosphorous acid with muriatic also, free from water. Corresponding products were also obtained by means of sulphur, consisting of dry sulphuric and muriatic acids; and the most remarkable circumstance attending these compounds, is, that they do not redden litmus paper, and manifest no marks of acidity till water is added to them.

In exposing potassium to these compounds, a violent detonation takes place, and Mr. Davy has some reason to hope that the muriatic acid suffers decomposition at the time, but he has not yet been able to collect the products for examination; and the elements of this acid, if separable, must remain a subject for future investigation.

An Account of a Method of dividing Astronomical and other Instruments, by ocular Inspection; in which the usual Tools for graduating are not employed; the whole Operation being so contrived, that no Error can occur but what is chargeable to Vision, when assisted by the best optical Means of viewing and measuring minute Quantities. By Mr. Edward Troughton. *Communicated by the Astronomer Royal.* Read February 2, 1809. [*Phil. Trans.* 1809, *p.* 105.]

The description of the method is preceded by some account of the steps by which the author acquired his present skill in the division of instruments, and by observations on the comparative merits of the respective methods employed by Bird, by the late Mr. John Troughton, and by other artists; after which Mr. Troughton proceeds to the account of his own, which, he says, was first suggested by the action of the perambulator. In the first place, the circle to be divided is to be turned on its inner and outer edges, to correct circles, in the most exact and careful manner. A roller is then adapted to its edge, having its diameter, as nearly as possible, one sixteenth that of the circle; and since perfect equality could not be directly obtained, the exterior surface of the roller is rendered slightly conical, by a difference of $\frac{1}{1000}$th of an inch in the diameters of its upper and under surface; so that by a small motion in the direction of its axis, some one part of its surface may be found perfectly adapted to its purpose. The roller itself being next divided into sixteen parts, each of these will correspond with $\frac{1}{256}$th of the whole circle; a number chosen on account of its being capable of continual bisection, although these divisions will coincide with a very small number of the ultimate divisions of the circle.

By means of two microscopes, one over the circle, and the other over the roller, the correct adaptation of the circle and roller to each other is first ascertained, or duly adjusted by raising or depressing the roller. An instrument for making dots, with uniformity, is next to be fixed at a due distance from the edge of the circle; and when one division of the roller is brought exactly under the wire of its microscope, the pointer being pressed down, makes the first dot at any point which may have been previously fixed upon. When the second division of the roller comes under its wire, a second dot is made in a similar manner, and so on till the whole 256 are completed, at intervals that are nearly equal. But it is not really of any consequence how erroneous they may be found by the very important process of examination which is next to follow, and which constitutes the intrinsic excellence of this method.

The dividing apparatus having next been removed, the circle is to be placed in the same position it is intended to have when employed for observation; and two microscopes are to be placed, at opposite sides, for the examination of the point of 180°. The dot at zero, and that at 180°, being first bisected, the circle is turned half round; and if when the zero point is bisected, that at 180° is found not to be bisected, the apparent error of its position is measured by the micro-

meter, and the half of that quantity is noted in a table + or —, according as its position is found forward or backward in the intended order of the future divisions. The four quadrants are next examined in the same manner, by removing the micrometer microscope to a position 90° distant. Half the observed error is again the real *difference of the two portions of semicircle compared*; and since the sum of the quadrants (though not accurately 180°) is known by the previous examination, the quantities themselves are known, and the real error of each quadrant is then noted. In a similar manner the succeeding bisectional points, at the distances from each other of 45°, 22° 30′, 11° 15′, 5° 37′ 30″, 2° 48′ 45″, and 1° 24′ 22½″, are successively examined; and the real errors of the several dots, from their true places, are computed and arranged in a table, so that by means of the dots themselves, together with their tabulated errors, the true places for the future divisions may be correctly known.

For the purpose of laying off these ultimate divisions, the circle is again placed in a horizontal position, and the roller is again applied to it. But as it would not be easy to divide the roller itself with sufficient exactness, a sector is added to the apparatus, having its radius four times that of the roller. This sector being fitted tight on the axis of the roller, moves with an angular velocity, which is sixteen times that of the circle; so that one of the former divisions of the circle is measured by an arc upon the sector of 22° 30′. But since the ultimate divisions are intended to be 5′ each, this sectorial arc must be divided into spaces of 80′ each; and of these spaces 16⅞ will be equal to 22° 30′, and will correspond with the true 256th part of the circle, or average space between the dots before laid down. The sector has consequently marked upon it eighteen intervals of 80′ each, the first and last of which are subdivided into eight parts of 10′ each. The fractional parts at each extremity are for the purpose of making the requisite coincidences with the former bisectional dots, and the intermediate sixteen divisions are the scale by which the true divisions are laid down.

Since this sector, though very correctly divided, may be liable to central error, its arc is made capable of a small adjustment, whereby 16⅞ths of its divisions are, by trial, made to correspond accurately with $\frac{1}{256}$th part of the circle.

For cutting the divisions, the same apparatus is employed as was used by Ramsden in his dividing engine, but originally invented by Hindley, of York. These, together with the two micrometer microscopes, constitute the whole apparatus to be employed.

The dividing point is first placed over that part of the circle at which the divisions are intended to be begun, while one of the microscopes is fixed accurately over the first of the 256 dots; and at the same time the first division of the sector is made to correspond with the wire of the second microscope.

The first division being now made, the circle is carried forward by a slow motion till the second division of the sector comes under the wire of its microscope, and the second division is now made upon

the circle. The succeeding divisions, to the sixteenth, are all made in the same manner. In the next place, the error of the second bisectional dot is to be set off by the micrometer head of the first microscope; and the contemporaneous coincidence of this dot, with that of the seventh of the succeeding small divisions of the sector, is to be observed, and then the sector must be moved backwards upon its axle sixteen divisions; so that it will have to move forward again by the motion of the circle one eighth of a division before the seventeenth division upon the circle is to be cut. The succeeding divisions follow in due course to the thirty-second, when allowance must be again made for the known error of the third dot, and the work proceeds in the same manner to the completion of the circle.

In the application of this method to the instrument now constructing for the Royal Observatory, which is to be divided on its edge, instead of having the divisions upon the face of the instrument, nothing new in principle is requisite, but merely a new position given to the roller, and other apparatus employed; but as that instrument may deserve a particular description, the author hopes to have an opportunity of giving an account of its construction, to the Society, at no very distant period.

A Letter on a Canal in the Medulla Spinalis of some Quadrupeds. In a Letter from Mr. William Sewell, *to* Everard Home, *Esq. F.R.S.* Read December 8, 1808. [*Phil. Trans.* 1809, *p.* 146.]

The canal, which is the subject of this letter, appears to have been discovered by the author in the year 1803, although no account has been given of it till the present description was drawn up at the request of Mr. Home.

From the extremity of the sixth ventricle of the brain in the horse, bullock, sheep, hog, and dog (which corresponds to the fourth ventricle in the human subject), a canal passes in a direct course to the centre of the spinal marrow, and may be discovered in its course by a transverse section of the spinal marrow in any part of its length, having a diameter sufficient to admit a large-sized pin; and it is proved to be a continued tube, from one extremity to the other, by the passage of quicksilver in a small stream in either direction through it.

This canal is lined by a membrane resembling the tunica arachnoidea; and it is most easily distinguished where the large nerves are given off in the bend of the neck, and at the sacrum.

A numerical Table of elective Attractions; with Remarks on the Sequences of double Decompositions. By Thomas Young, *M.D. For. Sec. R.S.* Read February 9, 1809. [*Phil. Trans.* 1809, *p.* 148.]

The attempts that have been made by some chemists to represent the attractive forces of chemical bodies by number, having been limited and hastily abandoned, some important consequences which

follow, from the principle of numerical representation, have been entirely overlooked.

Although there may be circumstances that will occasion exceptions to general rules, it appears that 100 numbers may be made correctly to represent nearly all the phenomena of the mutual action of 100 different salts, which, if described separately, would require about 5000 separate articles.

The author, having lately paid much attention to some of the principal facts in chemistry and pharmacy, has attempted the investigation of a series of numbers adapted to this purpose, and has succeeded in representing nearly 1500 cases of double decomposition enumerated by Fourcroy, with the exception of not more than twenty cases; and although it cannot be expected that these numbers are accurate measures of the forces they represent, yet they may be supposed to be tolerable approximations; for if any two of them be near the truth, the rest cannot be very far from it.

Dr. Young, however, observes, that if attractive force, which tends to unite any two substances, may always be represented by a constant quantity, it will follow, upon general principles, independent of any further hypothesis, that all known facts on this subject may be arranged in an order not liable to further alteration, in such a manner as to enable us to compare, with facility, a multitude of scattered phenomena. For if each force be constant, it follows that there must be a sequence in simple elective attractions, and palpable errors may thereby be detected in the common tables; for instance, in the four compounds resulting from the union of phosphoric and sulphuric acids with magnesia and ammonia, either the order of the acids, or the order of the bases, must be the same, otherwise the same force may be shown to be both greater and less than another.

The author observes, secondly, that there must be an agreement between the simple and double elective attractions; for if fluoric acid stands above the nitric under barytes, and below it under lime, the fluate of barytes cannot decompose nitrate of lime.

The author makes a third observation (which is less obvious), that there must be a continued sequence in the order of double elective attractions, and accordingly that between any two acids the several bases may be arranged in such an order, that any two salts will decompose each other, unless each acid be united to that base which stands nearest to it in the series; and a similar arrangement will obtain for the acid between any two bases. In forming tables of this kind from the cases collected by Fourcroy, the author has been under the necessity of rejecting some facts that were contradictory to others; and in admitting some which were not consistent with numerical representation, he has taken care to notice such inconsistency, and by notes of interrogation, or otherwise, to mark whatever remains in doubt. For the purpose of assisting the memory in retaining so numerous a series of facts, the author has contrived to express, in fifteen Latin hexameters, as many as 1260 cases of double affinity.

Account of the Dissection of a Human Fœtus, in which the Circulation of the Blood was carried on without a Heart. By Mr. B. C. Brodie. *Communicated by* Everard Home, *Esq. F.R.S.* Read February 16, 1809. [*Phil. Trans.* 1809, *p.* 161.]

Mr. Brodie was induced to draw up the account of this case, although other instances are already recorded, because the child differed much less from the natural formation than usual.

Twins were produced, both still-born, at the seventh month of pregnancy. The placenta was not preserved; but it was remarked that the chords belonging to the two children were, at their attachment, distant about three inches from each other. In one of these children nothing preternatural was observed. The other was distended, and disfigured with fluid contained in two cysts under the common integuments of the neck and thorax; but when the fluid was evacuated, the form was nearly natural, with the exception of a hare lip, and a deficiency of some of the toes and fingers. In the brain also, and nervous system, nothing unusual was observed. But in the thorax there was no heart, thymus gland, or pleura; and the substances corresponding to lungs, on each side, at the bifurcation of the trachea, were no more than one third of an inch in diameter, the thorax being filled with a dense cellular substance.

The diaphragm was merely membranous. The stomach had no cardiac orifice; the intestines were shorter than natural; and there was no omentum, no liver, and no gall-bladder.

At the navel entered two vessels, an artery and a vein; the former passing along, with the urachus, to the left groin, gave off the external and internal iliacs, and then passing upwards, joined the right iliac and became aorta, having the usual branches to the viscera and parietes of the abdomen; and when it reached the upper part of the thorax, it sent off the two subclavian arteries, and then divided into the two carotids, without forming any arch. The course of the veins was equally simple; but its communication with the navel was from the right groin, instead of passing along with the artery on the left side. In the whole course of these vessels there could be discovered no direct communication between the venous and arterial systems, as usual, but merely the union at their capillary extremities, at each termination, in the fœtus, and in the placenta; "so that the placenta must have been at once the source and termination of the circulation, and the blood must have been propelled by the action of the vessels only:" and although the circulation, under these circumstances, must be supposed unusually languid, it must be remembered that in this case the whole blood of the fœtus was exposed to the influence of the arterial blood of the mother, instead of that portion alone which usually branches from the general arterial system.

Various cases (all twins) are next cited by the author, from Mery, from Le Cat, and from Dr. Clarke, of fœtuses born without heart; and it is remarked, that all of these were smaller and less perfect than the present subject, which, in fact, was fully equal to the other fœtus of the same age with itself, in which the heart was perfect.

On the Origin and Formation of Roots. In a Letter from Thomas Andrew Knight, *Esq. F.R.S. to the Right Hon. Sir* Joseph Banks, *K.B.P.R.S.* Read February 23, 1809. [*Phil. Trans.* 1809, *p.* 169.]

The object of this paper is to show, that the roots of trees are always generated by the vessels which pass from the cotyledons of the seeds, or from the leaves through the leaf-stalks and bark, and that they never spring immediately from the alburnum.

The radicle, which proceeds from the seed, appears to the author to differ from other roots in its mode of growth, since it elongates, by interstitial increase, like the intervals between the buds in the succulent annual shoot; but roots, on the contrary, elongate only by new parts added to their extremity, and never by the extension of parts previously formed.

The proper roots, which come first into existence, spring from the point of the radicle; and since there is at that time no alburnum, it is evident they must arise from some other source.

At first they consist solely of cellular substance, within which cortical vessels are next generated; by these the alburnum is subsequently deposited, in the form of wedges, meeting in the centre.

If a portion of bark be removed from a vine, in a circle, round the stem, and any wet substance be applied to it, roots are soon emitted from the upper edge of the decorticated space; and when the alburnum dies so as to obstruct the progress of sap through it, buds are usually protruded from the lower edge, but never from the upper; the roots deriving their matter from the fluid that descends through the cortical vessels, and buds from the ascending sap.

In some varieties of the apple-tree, Mr. Knight observes, there are many rough excrescences on the trunks and branches, which, under different circumstances, form either buds or roots, and these varieties are accordingly very easily propagated by cuttings. When such excrescences had begun to form upon some trees of two years old, mould was applied to some of them in the spring, and roots were found to form early in the summer. But when mould was applied to other trees of the same age and variety, from which the top had been cut at a short distance above the excrescence, no roots were emitted for want of descending sap, but buds were formed instead.

The author observes, that both alburnum and bark contain true sap; but whether that which descends to form roots differs essentially from that which ascends to form buds, he thinks it nearly impossible to decide: he is, however, much more disposed to attribute the formation of different organs to the different action of the vessels, than to any difference of the fluids from which they are formed.

After alburnum has been formed in the roots, it then has the power of producing buds from its upper extremity, as well as fibrous roots from its lower extremity. The continuance of the entire root in the state of alburnum, appears owing to moisture; for if the mould be taken away so as to expose part of a root to the air, that part is subsequently found to contain heart wood.

The formation of buds from the potatoe; beneath the soil, may ap-

pear an exception to the general rule respecting buds and roots; but the author observes, that the tuber differs but little from a branch which has dilated instead of extending itself. The runners, which give existence to the tubers beneath the soil, are very similar in organization to the stem of the plant; and if exposed, readily emit leaves, and perform all the functions of the stem; and, on the other hand, Mr. Knight has shown, in a former memoir, that the buds on any part of the stem may be made to produce tubers similar to those formed beneath the soil; but he has never, under any circumstances, been able to obtain tubers from the fibrous roots of the plant.

Many naturalists have imagined the fibrous roots of all plants to be of annual duration only, because those of bulbous and tuberous plants certainly are so; but Mr. Knight observes, that the organization of trees is extremely different; and he has not found any portion of their roots to be deciduous.

On the Nature of the intervertebral Substance in Fish and Quadrupeds. By Everard Home, *Esq. F.R.S.* Read February 23, 1809. [*Phil. Trans.* 1809, *p.* 177.]

The author, having observed a new species of joint in the *Squalus maximus* of Linnæus, takes occasion to trace the successive gradations of a similar structure, through various kinds of fish, to the more remote resemblance to be found in quadrupeds and in men.

In the Squalus each joint of the spine approaches, in some measure, to that which is termed the ball and socket joint, as a concave surface of each vertebra is applied to a ball; but the ball, in this instance, is not, as in other cases, a smooth surface covering a solid bone, but a collection of fluid contained in a bag that is nearly spherical, round which the concave surfaces of the vertebræ are moved.

In a fish of thirty feet in length, the diameter of the body of one of the largest vertebræ measured seven inches; the quantity of fluid in one of the cavities amounted to three pints; the ligamentous substance, which unites the vertebræ, being nearly one inch in thickness, externally very compact and elastic, but internally possessed of but little elasticity.

The elasticity of these ligaments preserves the straitness of the spine when it is not acted upon by the muscles, or by other external force; and though the extent of motion, in any one joint, must be small, their number affords considerable latitude of motion.

Since the vertebræ, in other fish, are found with concavities in each surface, it was natural to expect a corresponding resemblance in the intervertebral structure; and in the skate this was found to be the case, and the cavity nearly spherical, as in the Squalus. In the common eel it is more oblong, the longitudinal diameter exceeding the transverse one by about one third.

In the sturgeon the structure varies considerably, as the cavities communicate with each other by apertures through the bodies of the vertebræ, which in this fish are cartilaginous rings, connected toge-

ther by ligament, and forming a tube communicating from one extremity of the spine to the other.

This species of intervertebral joint, which thus appears common to the fish tribe, is not found to obtain in the whales, as their structure in this, as in many other respects, is the same as that of quadrupeds, but is more distinctly visible, from the vast size of the parts. In them the intervertebral substance is arranged in concentric circles, connected by transverse fibres, the external layers being very firm and compact; but the interior become successively softer, till in the centre there is a soft pliant substance, more like jelly than an organized body, corresponding in its use to the incompressible fluid in fish.

In the bullock, sheep, deer, monkey, and man, the structure corresponds with that of the whale; but in the hog and rabbit a cavity was observed, with a smooth internal surface extending through half the diameter of the vertebræ; so that the structure in these animals imitates that of fishes, though not for any obvious purpose.

In the alligator the several joints are regularly articulated with capsular ligaments, and are lubricated with synovia. In the snake there is a regular ball and socket joint between every two vertebræ; so that the means employed for the motion of the back-bone in different animals, comprehends almost every species of joint.

Mr. Home's paper has annexed to it an appendix, by Mr. William Brande, giving an account of the chemical analysis of the fluid contained in the intervertebral cavity of the *Squalus maximus*.

Its specific gravity was found to be ·1027. It was not coagulated by heat.

No precipitation was occasioned by infusion of galls, or of catechu; nor was any change produced by alcohol.

But oxymuriate of mercury, muriate of tin, nitrate of silver, and acetate of lead, threw down copious precipitates.

From the effect of these re-agents, it appears to Mr. Brande, that the fluid contains neither gelatine nor albumen; but when the fluid was evaporated to half its bulk, pellicles began to form on the surface, indicating the presence of a variety of animal matter, which the author considers as *mucus* or *mucilage*, but which, under certain circumstances of evaporation, is capable of being converted into a modification of gelatine or albumen.

On Platina and native Palladium from Brazil. By William Hyde Wollaston, *M.D. Sec. R.S.* Read March 22, 1809. [*Phil. Trans.* 1809, *p.* 189.]

Until a portion of platina was lately discovered by M. Vauquelin, in some silver ores from Estremadura, the whole of the platina known in Europe was derived from the Spanish possessions in South America, and had very uniformly the same appearance, differing solely in the magnitude of the grains.

A third variety having lately been received from Brazil, the author

thought it deserved particular examination, although the quantity which he could obtain was too small for accurate analysis.

The appearance of this mineral is whiter than Peruvian platina; the grains are rougher and more angular, being evidently fragments of larger masses, very little worn at their surfaces. When examined by solution and precipitation, the greatest part of the grains appeared to be platina nearly pure, as they are free from iron, which forms a considerable part of the Peruvian ore; and apparently free from the several metals, which have within these few years been discovered in that mineral; but they contain, on the contrary, a small quantity of gold, which is not contained in the grains of Peruvian platina.

The author discovered also, among the grains of native platina, a few fragments of native palladium, which he describes as resembling, in the whiteness of their colour, the grains of platina, but differing from them in presenting an appearance of fibres diverging from one extremity. These grains are readily detected by their solubility, and by the red colour of the solution: that they consisted of palladium, was proved by precipitation with prussiate of mercury, or green sulphate of iron, as well as by their fusibility by assistance of sulphur. It is remarked, however, that these grains are not absolutely pure, but contain a very small quantity of platina, which, by its redness when precipitated, seems to be contaminated by iridium.

On a native Arseniate of Lead. By the Rev. William Gregor. *Communicated by* Charles Hatchett, *Esq. F.R.S.* Read April 13, 1809. [*Phil. Trans.* 1809, *p.* 195.]

The mineral of which this account is given was raised in a very rich copper-mine called Huel-Unity, in the parish of Gwennap, having been found at the depth of fifty fathoms, at the junction of two small lodes or veins. This ore is mixed with some native copper, very rich gray copper, and black copper ore.

It crystallizes in the form of a hexahedral prism, terminated in general by a plane, but sometimes by a taper six-sided pyramid. The colour is generally a shade of yellow, but sometimes wine-yellow, like the Brazilian topaz, and sometimes as dark as brown sugar-candy. The hardness varies, and is sometimes sufficient to scratch flint-glass. The specific gravity at 50° temperature is 6·41.

Being exposed to heat upon a gold spoon, it melts into a brownish-yellow mass, and remains unaltered in a state of ignition. But if heated upon charcoal, it is rapidly decomposed, arsenical vapours being extricated, while the lead is reduced to its metallic state.

The mode of analysis adopted by the author consisted in reducing the ore to a fine powder, and decomposing it by a solution of pure potash, with due precaution to avoid the solution of lead by the alkali along with the arsenic acid. The arseniate of potash was decomposed by nitrate of lead, which gave an arseniate of lead, consisting of known proportions, from which the quantity of arsenic acid in the ore was found to be 26·4 per cent.

The oxide of lead, which had been deprived of its arsenic acid by the potash, was then dissolved in nitric acid, and precipitated by sulphate of soda in the state of sulphate of lead, from which the quantity of lead in the ore proved to be 69¾ per cent.

Mr. Gregor has found only one specimen in which the proportion of lead to the acid was materially different. In this instance the oxide of lead was 71·45, and the acid 23·88, instead of being, as before, 69¾ and $26\frac{4}{10}$.

Beside these ingredients, the ore also contains a portion of muriatic acid; and the author has also detected small but variable proportions of iron and silica.

The quantity of muriatic acid was ascertained by solution of the ore in nitric acid, and precipitation as usual by nitrate of silver. But Mr. Gregor found it necessary to take certain precautions; for if the solution be made with much heat, part of the muriatic acid is lost by boiling; and if the solution be too concentrated, an arseniate of silver is precipitated along with the muriate, and will then require to be separated, either by solution of it in nitric acid, or by means of its insolubility in pure ammonia, which dissolves the muriate.

In order to determine decisively the nature of the principal acid present in this ore, Mr. Gregor decomposed a portion by sulphuric acid, and, after evaporation of the fluid poured off, reduced a part of the acid upon charcoal. Part was dissolved in water, and precipitated titanium from sulphate of titanium; part was neutralized with soda, and occasioned a brick-coloured precipitate from nitrate of silver, and a reddish yellow precipitate from nitrate of mercury.

From the whole of the experiments detailed in the paper, the author concludes that 100 parts of the ore contain 69·76 oxide of lead, 26·40 arsenic acid, 1·58 muriatic acid; and that the silica and oxide of iron are not essential to its composition.

An anatomical Account of the Squalus maximus (*of* Linnæus), *which in the Structure of its Stomach forms an intermediate Link in the Gradation of Animals between the Whale Tribe and Cartilaginous Fishes. By* Everard Home, *Esq. F.R.S.* Read May 11, 1809. [*Phil. Trans.* 1809, *p.* 206.]

The fish described in this account was caught in a herring-net at Hastings, from whence such parts as were more particularly deserving of notice were brought to London for further examination.

It was a male, thirty feet six inches long, and nine feet broad, from the tip of the dorsal fin to the middle line of the belly.

The skin was of a light slate-colour, and though as rough as a new file in the direction from the tail to the head, yet as smooth as satin in the opposite direction.

The mouth was about five feet wide, with six rows in each jaw of small conical teeth, rather curved inwards.

The nostrils were placed on the edge of the upper lip.

The eyes very small, with pupils perfectly round.

Half way between the eye and the gills was an orifice and canal leading to the mouth. The gills five in number on each side.

The fins, and also their situation, are particularly described.

Adjacent to the anal fins are placed two holders for the purpose of grasping the female, terminated by a flat, sharp, bony process five inches long, which moves on a joint, and is, in fact, the termination of a series of parts corresponding to the pelvis, femur, tibia, and foot of quadrupeds.

The pectoral fins also correspond in some measure to the anterior extremities, and are connected by cartilages, which answer the same purposes as the scapulæ and sternum of quadrupeds.

The heart was not larger than that of a bullock, with three valves at the origin of the pulmonary artery, three at the entrance of the aorta, and also two sets more, of three each, in the course of the artery, at a short distance from each other.

The stomach contained several pails full of pebbles, a quantity of mucus, and a small portion of substance that looked like the spawn of the oyster.

Beside the cardiac and pyloric portions of the stomach observable in other sharks, there was a globular cavity communicating with the pyloric portion by a very small orifice, and by another, equally small, with the intestine.

The liver of this fish yielded about three hogsheads of oil. The vessels of the liver were large enough to admit a man's arm. The bile is conveyed direct to the intestine by twelve hepatic ducts, for there is no gall-bladder.

Although the Squalus here described resembles, in many respects, the tribe of Sharks, it is observed to differ essentially in the form of its stomach, which is intermediate between that of the shark and whale.

In the modes of generation, also, as well as in the stomachs, a series of gradations may be observed from whales through the squalus, sharks, rays, and skates, to the proper fishes; but this inquiry will form the subject of a future communication.

Mr. Home closes the present account by such particulars as he could collect concerning a large fish thrown ashore on one of the Orkneys, and described as a sea-snake by those who had seen it half putrid and half devoured by sea-fowl; but it was ascertained by Mr. Home to be in reality another specimen of the same Squalus as that above described.

On an Improvement in the Manner of dividing astronomical Instruments. By Henry Cavendish, ***Esq. F.R.S.*** Read May 18, 1809. [*Phil. Trans.* 1809, *p.* 221.]

The use of the common beam-compass for dividing having been justly objected to, on account of the danger of bruising the divisions which have been made, by replacing the points of the compass into

them, the author proposes a means of obviating that inconvenience, by substituting a microscope instead of one of the points; and he describes a method of proceeding, in which there is no need ever to set the other point into any division already made.

The beam to be employed for this purpose must have a fixed point at one extremity, and at the other a centre of motion, round which the length of the beam may revolve as radius. A microscope is to slide in a groove along the middle to any required distance from the point; and in order that these may both be over the circle at the same time, the centre of motion must be capable of adjustment, that it may be fixed at a greater or less distance from the centre of the circle, according to the magnitude of the arc intercepted between the point and microscope.

In dividing by continual bisection, the microscope is first to be removed from the point to a distance nearly equal to the chord of the half-arc; and when the centre of motion has been duly adjusted, and the wire of the microscope is made to bisect the dot at one extremity, a faint scratch must be made with the point.

The beam having next been turned half round, and the dot at the other extremity brought under the wire of the microscope, a second scratch is made with the point, which, if the distance has been taken, will be very near the former; and the wire of the microscope will easily be placed midway between them in the further process of bisection, which is again performed in the same manner, after the position of the microscope and of the centre of motion have been duly altered.

In laying down the real divisions from the marks thus made, the centre of motion must be so placed that the whole length of the beam may become a tangent to the circle; and when the microscope has been fixed close to the point, and the first dot brought under it, the first division is to be marked, and the rest in succession till all are made.

Since the entire arc of a circle cannot be divided to degrees without trisection and quinquesection, Mr. Cavendish describes three methods of quinquesection, which it would be difficult to render intelligible without reference to the figures which accompany his paper; and he also makes an estimate of the comparative accuracy attainable in bisection, trisection, and quinquesection.

As it would be difficult to place the centre of motion accurately, so that the point and axis of the microscope shall both be in the circle in which the divisions are made, it becomes necessary that the wire of the microscope should be placed truly at right angles to the length of the beam; for then, although the point of intersection of the circle with the wire of the microscope is not accurately in the middle of the wire, still, when the beam is reversed, the point of intersection will lie at an equal distance on the opposite side of the centre, and will consequently be at a given distance from the fixed point of the compass.

In describing the apparatus, Mr. Cavendish has not entered further than was necessary to explain the principle, and has left the completion of it to the skill of any artist who may choose to adopt it.

On a Method of examining the Divisions of astronomical Instruments. By the Rev. William Lax, *A.M. F.R.S.* Lowndes's *Professor of Astronomy in the University of Cambridge. In a Letter to the Rev. Dr.* Maskelyne, *F.R.S. Astronomer Royal.* Read June 1, 1809. [*Phil. Trans.* 1809, *p.* 232.]

Since the utmost precision in making astronomical observations, and in reading off the indication given by any instrument, will be of no avail if the instrument itself be not divided with proportional accuracy, the author felt the importance of *estimating* the *probable* amount of errors that might occur in Bird's method of dividing by continual bisection, and has also contrived a method of examining the divisions of any circle, and of measuring, within certain limits, the actual errors in every part of it.

The apparatus by which this examination is effected, is first minutely described, and consists of a brass arc, rather more than 90° in length, placed concentric with the circle to be examined, and firmly attached to the frame which supports the microscopes. On this arc an upright pillar is made to slide, carrying a micrometer microscope, which may thus be fixed at any distance not exceeding 90° from one of the microscopes belonging to the circular instrument; and as the position of the microscope is inclined, it may be made to point to the same division upon the circle that is under the micrometer itself.

In the process of examination which follows, the position of the point of 180° having been first ascertained by means of the opposite micrometers belonging to the instrument, the arcs of 90° on each side are next examined by the moveable microscope, and the errors noted accordingly + or −. The microscope is then placed at the distance of 60° from the micrometer, and the first sextant is thus compared with every succeeding arc of 60° in the circle; and in the same manner, the first octant is compared with every succeeding arc of 45°, and the first arc of 30° with so many of the succeeding arcs of 30° as are necessary for determining each 15° of the whole circle.

The next intervals employed by Mr. Lax are those of 5° and 3°, from which, and from their multiples, the value of 1°, 2°, and 4°, are derived; and, in a similar manner, all the succeeding intervals down to the smallest interval to which the circle happens to be divided.

However, since the method of examination itself is liable to some error, the author computes the extent to which this may possibly amount; and upon a circle of one foot radius, he finds the greatest aggregate error to which he could be liable, in points most remotely deduced, might be 9″·63: but in a circle of three feet radius, the error would be reduced to 3″·21; and with glasses of higher magnifying power, and by frequent repetition of the reading off, the true

position of every point might be determined with sufficient exactness for every practical purpose.

The time required for such an examination is estimated to be about ninety-eight hours, and the labour, no doubt, is very considerable; but when the errors thus ascertained have been duly noted in a table, Mr. Lax considers the utmost pains that can be bestowed upon any instrument to be amply compensated by the confidence given to every subsequent observation by means of it.

It is also proposed occasionally to obviate the effects of unequal expansion in any particular observation, by comparing the arc by which any angle has been measured with several succeeding equal arcs, until the multiple exceeds the whole circumference, and thereby includes the opposite errors, which arise from this cause in different parts of a circle, and correct each other.

On the Identity of Columbium and Tantalum. By William Hyde Wollaston, *M.D. Sec. R.S.* Read June 8, 1809. [*Phil. Trans.* 1809, *p.* 246.]

The author having received specimens of the Swedish mineral tantalite, containing the metal called Tantalium, by Mr. Ekeberg, was desirous of ascertaining whether that metal might not be the same as columbium, which had been discovered a short time before by Mr. Hatchett; and for that purpose he procured some oxide of columbium from Mr. Hatchett, and also a fragment of the mineral in the British Museum, originally analysed by Mr. Hatchett.

He describes the external resemblance to be such, that one might be taken for the other; but observes, that the columbite is rather more brittle than tantalite.

By analysis, also, he finds them to consist of the same three ingredients; namely, a white oxide, iron, and manganese.

To separate these substances, the mineral is powdered and fused with carbonate of potash and a small proportion of borax. The iron and manganese may then be dissolved, along with the salts employed, by muriatic acid, and the oxide of columbium or tantalium remains as a white powder for further trial of its properties.

Five grains of columbite being thus treated, left four grains of white oxide; and the solution yielded three fourths of a grain of iron, and one fourth of a grain of manganese.

Five grains of tantalite, by the same treatment, left four grains and a quarter of oxide, half a grain of iron, and two tenths of a grain of manganese.

The white oxides obtained from each of these minerals appear to the author to have precisely the same properties.

They are each soluble by means of about eight parts of potash.

They are both very imperfectly soluble by means of soda.

They are both insoluble in nitric, muriatic, succinic, and acetic acids.

They are both very sparingly soluble in strong sulphuric acid

while boiling; but they are nevertheless both perfectly soluble in oxalic acid, in tartaric acid, or in citric acid.

They are both precipitated of an orange colour by infusion of galls, but are not precipitated by that re-agent if a considerable excess either of alkali or acid prevail in the solution.

As a further agreement in their properties, it is added, that neither of them is precipitated by prussiate of potash or by hydrosulphuret of potash.

From these experiments, although a great difference which subsists between the specific gravities of the two minerals cannot be very satisfactorily explained, the author is satisfied that the American and Swedish minerals, in fact, contain the same metal.

Description of a reflective Goniometer. By William Hyde Wollaston, *M.D. Sec. R.S.* Read June 8, 1809. [*Phil. Trans.* 1809, *p.* 253.]

The instrument here described by the author is designed to obviate the inconvenience which has been found in attempting to measure any small crystals by the instruments hitherto used for that purpose.

When a surface is so small as one fiftieth of an inch in breadth, it becomes extremely difficult to apply the short radius of a goniometer to it with correctness. But since a surface of that magnitude may reflect a very brilliant light, the reflected ray may be employed as radius, and may at pleasure be taken of such a length that the angles of small crystals can be known with as much precision as those of the largest surfaces.

The crystal being attached to a horizontal axis, with its edge in the line of the axis, one of the surfaces is made to reflect some bright light to the eye; and, while the eye is retained steadily in the same place, the axis is turned till the second surface reflects the same light, and is consequently in the same position. The number of degrees through which the axis has turned being the supplement to the required angle, the angle itself is indicated by the graduations of a circle which moves with the axis; but the complete construction of the instrument cannot be distinctly understood without reference to a figure that accompanies the paper.

Since any inaccuracy in placing the crystal would occasion some error by parallax in this method of using the instrument, the author describes a second method, by which all error may be entirely obviated.

By placing the crystal so that the image of some distant object is brought to correspond with some other object by one of its surfaces, the position of that surface is determined with precision, and the second surface may be brought round to the same position with the utmost accuracy.

With this instrument the author has remarked an error in the supposed angle of the primitive crystal of carbonate of lime, which, instead of being 104° 28′ 40″, as it is now considered by writers on crystallography, appears to the author to be correctly 105°, as it was formerly measured by Huygens and by Sir Isaac Newton.

Continuation of Experiments for investigating the Cause of coloured concentric Rings, and other Appearances of a similar Nature. By William Herschel, *LL.D. F.R.S.* Read March 23, 1809. [*Phil. Trans.* 1809, *p.* 259.]

In the former part of this paper the author had pointed out various methods of producing coloured rings between surfaces in contact, and had proved that no other surfaces are concerned in their formation; and he now proceeds to show, that, by varying the figures of the surfaces, a corresponding change of the form of the rings will be observed. As a spherical surface applied to a *plane* surface produces circular rings, so, when it is applied to a *cylindrical* surface, the rings become ellipses; and when the cylinder is applied to a plane, the ellipses become straight lines, and irregular curves occasion irregularity in the form of the rings. But Dr. Herschel is of opinion, that plane surfaces, applied to each other at any extremely small angle, cannot exhibit any such appearances of colour unless they are disfigured by unequal pressure.

The author next examines the circumstances of the prismatic blue bow observed by Newton, as the limit to the perfect reflection that occurs in the interior of a prism when light is much inclined to its surface; and Dr. Herschel calls this the critical separation of the colours. He also examines the prismatic red bow observable at the same critical inclination, when the transmitted light prevails over that which is reflected; and remarks the change from red to blue, occasioned by changing the direction of the light.

The next phenomena observed are the coloured streaks that are produced adjacent to these bows when the reflecting surface of the prism is in contact with another surface; the streaks being straight when the surfaces are plain, or curved, according to their different kind or degree of curvature. In the experiments which follow, this effect of a surface in contact with the prism is employed as a criterion for distinguishing more clearly the acting surface.

Various more complicated appearances are next examined; in which, by reason of second reflection, the origin of the bows is less distinct, or in which two bows may be seen at once from the particular form of the prism.

In the formation of bows, it is shown, that only one surface of the prism is concerned by roughening other surfaces, and by applying to them various glasses, without preventing the appearance of the bows.

Since the streaks which are seen beneath the blue bow, when a plane surface is in contact with a prism, contain all the prismatic colours, it is evident that the less refrangible, after having been transmitted, are reflected back by the contiguous surface, and then re-enter the prism.

Dr. Herschel next proceeds to delineate, from strict computation, the course of the several rays; and, by a diagram drawn upon an enlarged scale, shows, that when light falls upon a pair of surfaces nearly in contact, and when single rays or small pencils are separated

at regular intervals from each other, then the rays, which emerge, and after reflection re-enter the same surface, will also be separated so as to present streaky appearances, in some of which the order of the colours will differ from that in others, agreeably to observation.

With respect to the curvature of the prismatic bows, *that* manifestly depends upon the position of the eye, as the lines of equal incidence form a cone, of which the eye is apex; and, accordingly, as the eye recedes from the prism, the bow becomes less curved, and for a small space will appear straight.

Dr. Herschel having remarked, when a plane surface is applied to a prism, that the appearance of the streaks that are seen in contact with the prismatic bow depends on what he has termed the critical separation of the rays, infers, that those rings of colours which are seen when a lens is laid upon a plane surface, depend also upon the same critical separation; the lens being in this case considered as a prism bent round into a circular form.

The several points of resemblance in the circumstances under which the bow-streaks from a prism, and the rings from contact of lenses, appear or disappear, or change their form or their colours, are next compared; and are considered by the author as decisive proof that each arise from the same critical separation at the boundary of prismatic reflection; and if the mere difference of refrangibility of the several colours is sufficient to account for the phenomena, it is inferred, that no alternate fits of easy reflection and easy transmission really exist as original properties in the rays of light.

An Account of a Calculus from the Human Bladder of uncommon Magnitude. By Sir James Earle, *F.R.S.* Read June 15, 1809. [*Phil. Trans.* 1809, *p.* 303.]

This calculus, which is considered as the largest on record, weighs 44 ounces avoirdupois. It was taken after death from the bladder of Sir James Ogilvie, who had submitted to a fruitless attempt to remove it by the usual operation of lithotomy, rather than prolong an existence extremely miserable from this among other consequences of a blow on his back thirty years before. The stone so completely filled the cavity of the bladder, that it was with difficulty taken out, although there was no real adhesion. Its texture was less compact than that of calculi in general, but agreed in appearance with that species which has been called the Fusible Calculus; and it was ascertained by Dr. Powel to contain the same ingredients as it was composed of, the ammoniacal phosphate of magnesia, with phosphate of lime.

On expectorated Matter. By George Pearson, *M.D. F.R.S.* Read June 15, 1809. [*Phil. Trans.* 1809, *p.* 313.]

Dr. Pearson's inquiry comprehends several varieties of expectorated matter, which he arranges under the following heads:

1. That which is semi-transparent blueish, and of the consistence of jelly.

2. What is transparent and fluid, as mucilage.

The 3rd is thick, opake, and straw-coloured, or white, and very tenacious.

4. Puriform matter secreted without breach of surface.

5. Matter consisting of the mixture of the 2nd and 3rd, or 2nd and 4th varieties.

6. Pus from vomicæ, arising from tubercles.

7. Pus from vomicæ, consequent on simple inflammation.

The author next treats more at large of the sensible or obvious properties, including smell, taste, weight relative to water, and appearance under the microscope.

The next division of the subject includes the agency of heat; the separation of a coagulum, or curd, at temperatures from 150° to 170°; the properties of the curd, and of the residuum which it leaves when burned; the saline contents of the fluid separated from the curd, and obtained by evaporation.

Under the same head of agency of heat is also included the process of distillation to dryness, the properties of the fluid distilled, and of the residuum.

The action of alcohol is next tried, both on the residuum left by distillation, and also on different kinds of expectorated matter in their fresh state.

Next follow experiments on the miscibility of these matters with water, and also a set of experiments made with acetous acid. After other miscellaneous experiments, the author concludes, that the various kinds of expectorated matter consist of the same ingredients, but that the proportion of these ingredients is somewhat different; that expectorated matter consists of water containing certain saline and earthy bodies, together with a coagulable albuminous oxide, amounting in general to between one sixteenth and one twentieth of the whole, but sometimes not exceeding one fiftieth part, and at others amounting to as much as one tenth.

The saline ingredients are stated to be,

Muriate of soda, varying from 1 to 2 in 1000 parts.

Potash, neutralized by animal oxide, 0·4 to 0·7.

Phosphate of lime, 0·4 to 0·5.

Ammonia, united probably to phosphoric acid.

Phosphate, perhaps of magnesia.

Carbonate of lime.

Indications of sulphate of lime.

Vitrified matter, perhaps with silica.

Oxide of iron in too small proportion to be estimated.

The last six substances scarcely amounting to one thousandth part.

The different consistence is ascribed to the different quantities of albuminous oxide. The thicker matter is also said to be less saline than that which is more fluid.

The author observes, that all the animal fluids contain potash, but

that pus does not contain so much potash or muriate of soda as is contained in the above expectorated secretions.

Dr. Pearson also thinks it much more probable that the circulating and secreted fluids are impregnated with potash, as he has observed, than with soda, as observed by others.

Finally, we are informed, that expectorated matter contains globules, which have not before been observed, and seem to the author to denote organization.

On the Attractions of homogeneous Ellipsoids. By James Ivory, *A.M. Communicated by* Henry Brougham, *Esq. F.R.S.* Read June 15, 1809. [*Phil. Trans.* 1809, *p.* 345.]

The theory of the figures of the planets involves two questions perfectly distinct from each other; first, the *figure* which a mass of matter would assume by the mutual attraction of its particles, combined with a centrifugal force, arising from rotatory motion; and secondly, the *force* with which a body so formed will attract a particle occupying any proposed situation. The latter is the subject of the present inquiry; and it is also limited to the consideration of homogeneous bodies bounded by finite surfaces of the second order.

This subject was first partially treated of by Sir Isaac Newton, who, in determining the attraction of spherical bodies, has also treated of other solids, formed by the rotation of curves round an axis, and of the attractions they exert upon bodies placed in the line of their axes. MacLaurin was the first who determined the attractions that such spheroids of revolutions exert on particles placed anywhere, either *in* or *within* their surfaces.

Le Gendre extended the same inquiry to particles *without* the surface of such solids of revolution.

La Place took a more enlarged view, and extended his researches to all elliptic spheroids, not formed by revolution, but such whose three principal sections are all elliptical; and he arrived at conclusions, with regard to them, similar to those of MacLaurin and Le Gendre.

But notwithstanding the ingenuity and skill displayed by La Place in this investigation, Mr. Ivory conceived that the inquiry might be simplified, and the results obtained more directly, by a method which forms the subject of the present communication; which, however, is of a nature not adapted for public reading.

Observations on Albumen, and some other Animal Fluids; with Remarks on their Analysis by electro-chemical Decomposition. By Mr. William Brande, *F.R.S. Communicated by the Society for the Improvement of Animal Chemistry.* Read June 15, 1809. [*Phil. Trans.* 1809, *p.* 373.]

Mr. Brande's paper consists of two parts; first, Observations on mucus and on the composition of liquid albumen; and secondly, On the composition of some animal fluids containing albumen.

The mucus contained in saliva in expectorated fluids, and in that from the oyster, were first examined; but since nitrate of silver and acetate of lead, which have been supposed to detect mucus, were found to act principally on the salts contained in them, it became necessary to employ other means for the removal of the salts; and the voltaic apparatus was applied for the purpose of extracting the alkalies at one pole, and the acids at the opposite. But there occurred a phenomenon that was wholly unexpected; as a considerable coagulation of albumen took place at the negative pole, which Mr. Brande (at the suggestion of Mr. Davy) is inclined to ascribe to the separation of alkali with which it was combined, and to which its solubility was owing.

It is observed, in confirmation, that when an egg is boiled for some time in water, the liquid becomes alkaline to tests, and still deposits, by electrization, a small quantity of albumen, which the alkali retains in solution.

The coagulation of albumen by acids is also ascribed to their superior affinity for the alkali.

For discovering the nature of the saline ingredients, the water in which some white of egg had been boiled and macerated, was electrified by a powerful battery, through the medium of a cup of water on each side. After the process had continued for one hour, the fluids were examined; that on the negative side contained a quantity of soda nearly pure, and that on the positive side a small quantity of albumen, with a little muriatic acid, but not enough to saturate the alkali.

The same means of analytic investigation being applied to other fluids, detected larger quantities of albumen than were discoverable by heat alone; as in saliva, in the mucus from the oyster, the mucus from the trachea, in bile, in milk, and in the liquor of Amnios: and hence the author is led to doubt whether mucus may not be a compound of albumen, either with muriate of soda or with excess of soda.

Hints on the Subject of animal Secretions. By Everard Home, *Esq. F.R.S. Communicated by the Society for the Improvement of Animal Chemistry.* Read June 22, 1809. [*Phil. Trans.* 1809, *p.* 385.]

The separation, by electric powers, of substances chemically united, suggests the possibility, that since the same power is known to exist in the torpedo and electrical eel, it might be the means by which secretion in all animals is effected.

Since in these fish the abundance of nerves connected with the electrical organs proves that this power resides in them, and since the arrangement of many nerves in animal bodies has evidently no connexion with sensation, it seems not improbable that these may answer the purpose of supplying and regulating the organs of secretion.

With a view to determine what changes could be produced in the blood similar to secretion, Mr. Brande applied the power of twenty-

four pair of four-inch plates of copper and zinc to blood, and extracted acid and alkali at the opposite wires.

A second experiment was made, with similar results, on blood still fluid, in the vein of an animal just killed.

A third experiment was made upon serum, with 120 plates highly charged, with the same result.

A fourth experiment was conducted in a similar manner, with 12 pair of plates, with similar results.

In a fifth experiment, 30 pair of plates, very weakly charged, also extracted alkali and acid from serum exposed to them.

Since powers so weak are capable of separating the constituent parts of blood, it is suggested that the weaker powers existing in animals may produce the same effect, and thus occasion all the different secretions, and modify albumen into the states of the different animal solids.

On the comparative Influence of Male and Female Parents on their Offspring. By Thomas Andrew Knight, *Esq. F.R.S. In a Letter to the Right Hon. Sir* Joseph Banks, *Bart. K.B. P.R.S.* Read June 22, 1809. [*Phil. Trans.* 1809, *p.* 392.]

During the very extensive series of Mr. Knight's endeavours to improve the varieties of fruit-trees, he has also been occupied in making correspondent experiments on the breeding of animals, and has always paid attention to the strong analogy which universally subsists between plants and animals in most points relating to generation.

Although the author's experiments have extended to many different species of fruit-trees, yet the greatest number, and those under the most favourable circumstances, were upon apple-trees. But as the results were all in unison, the instances here adduced are from the apple alone.

Linnæus conceived the character of the *male* to predominate in the exterior both of plants and animals: but Mr. Knight's observations have led him to form a different conclusion; for he remarks, that seedling plants and the young of animals inherit much more of the character of the *female*.

Seeds from cultivated apple-trees, impregnated by the Siberian crab, produced larger fruit than those from the crab impregnated by stamina from the cultivated fruit; but the quality and flavour of the fruit appeared to inherit, in a greater degree, the qualities of the male.

In consequence of the frequent intermixtures that have taken place in the breeding of domesticated animals, there is often little resemblance to either parent; but it is observed, that the dimensions of the offspring are regulated principally by those of the female, and that a corresponding length of legs appears especially necessary for accompanying the parent in flight. But unless the male parent be proportionally strong, the legs of the offspring may be too long in

proportion to the strength which it will ultimately attain, and it may be ill adapted to the purposes of labour.

Mr. Knight remarks, that with respect to sex also, the influence of the female entirely predominates, as particular females will produce all their offspring of one sex, either male or female; but by attending to the numerous offspring of a single bull, ram, or horse, he has never witnessed any considerable difference in the numbers of the two sexes.

The size and form of the eggs of oviparous animals being dependent wholly on the female, regulate of course the size of the offspring, and in this respect resemble the seeds of plants; but their formation, nevertheless, appears to depend on different laws; for the eggs of birds, fishes, and insects, attain their full size in total independence of the male; but in seeds, on the contrary, the whole internal organization depends on the influence of the male. For though a gourd may produce fruit, apparently perfect, without impregnation, and although even the seed-coats acquire their natural size and form, these coats are perfectly empty, without the slightest vestige of cotyledons or plumula, or anything that appears to correspond with the internal organization of a complete seed.

On the Effect of westerly Winds in raising the Level of the British Channel. In a Letter to the Right Hon. Sir Joseph Banks, *Bart. K.B. P.R.S. By* James Rennell, *Esq. F.R.S.* Read June 22, 1809. [*Phil. Trans.* 1809, *p.* 400.]

The recent loss of the Britannia East Indiaman having appeared to the author intimately connected with those observations which he formerly communicated to the Society concerning a current that occasionally prevails to the westward of Scilly, he has thought the circumstances deserving of particular inquiry, as it may be of use to record them as a warning against dangers which may probably be avoided.

During the prevalence of westerly winds, it is well known that they occasion the height of the tides to be greater in the southern parts of Great Britain, even as much as ten feet, in violent storms from that quarter; and it has also been remarked, that the flood-tide runs an hour or more longer than at common times; or, in other words, that a current overcomes the ebb-tide. And since the direction of this current must be influenced by the form of the adjacent shores, it is evident that the shore in the neighbourhood of Boulogne, which tends very directly northward, will cause a northerly current through the Straits of Dover.

It appears to have been under these circumstances, that the Britannia was lost. Having been driven up the Straits by a violent gale from the south-west in thick weather, which prevented a view of the lights, the pilot was left entirely to the reckoning and the lead; but as he was not aware of the unusual current that prevailed, he was driven unexpectedly on the back, or eastern side of the Goodwin Sands.

On Respiration. By William Allen, *Esq. F.R.S. and* William Hasledine Pepys, *Esq. F.R.S.* Read June 22, 1809. [*Phil. Trans.* 1809, *p.* 404.]

Experiments on Ammonia, and an Account of a new Method of analysing it, by Combustion with Oxygen and other Gases; in a Letter to Humphry Davy, *Esq. Sec. R.S. &c. from* William Henry, *M.D. F.R.S. V.P. of the Lit. and Phil. Society, and Physician to the Infirmary, at Manchester**. [*Phil. Trans.* 1809, *p.* 430.]

Dr. Henry having detected a fallacy in the experiments lately communicated to the Society, in which oxygen gas appeared to be evolved from ammonia by electric discharges, has repeated the experiments with more care, and is now satisfied that no portion whatever of oxygen gas can be separated.

He next endeavoured to detect the production of water, which would equally establish the existence of oxygen as a constituent; and though mere electrization did not enable him to discover the smallest quantity of moisture, he hoped, by exposure of the gas to a freezing mixture, that it might be detected.

But although, by a cold equal to zero of Fahrenheit, a slight degree of moisture was perceptible, even this appearance varied in degree; and Dr. Henry was ultimately convinced, that even this arose from the mercury, or from some extraneous source.

With regard to the quantity of nitrogen and hydrogen gases evolved from ammonia by electrization, Dr. Henry is of opinion that they have been underrated by Mr. Davy, and that the proportion they bear to each other is not exactly what it has been represented. According to Dr. Henry, 100 of ammonia give 198·78; and the proportion of hydrogen to nitrogen is as 71·4 to 28·6 in the 100.

On account of the tedious labour of decomposing any quantity of ammonia by the mere repeated shocks of electricity, the author was induced to seek some new method of operating, and after various trials succeeded to his satisfaction, by detonation with oxygen. When more than three parts of oxygen gas are mixed with one of ammonia, or when so little as only half of oxygen is mixed with one of ammonia, the mixtures are not combustible; but when any intermediate quantity of oxygen is employed, the ammonia is consumed in proportion, and entirely disappears if the oxygen is more than double its bulk.

Dr. Henry, however, discovered an important advantage of using an under proportion of oxygen; for though the whole of the hydrogen is not then consumed, yet the whole of the ammonia is decomposed; for after detonation the remaining hydrogen, and the whole of the

* This letter, in its original form, was read to the Society, May 18th, 1809; some new observations were added, and some corrections furnished by the author, in consequence of subsequent experiments made in June; it was transmitted to the Secretary for publication July 10.

nitrogen, are in a gaseous state, composing a mixture which may be detonated with a fresh addition of oxygen; and in this way all the hydrogen may be saturated with oxygen, and the nitrogen may be obtained as a final result of the process.

From a set of experiments thus conducted, the proportion of hydrogen to the nitrogen in ammonia was pretty uniformly 70·6 to 29·4; but there was some irregularity in the entire quantities of both, the highest results being as much as 200·6, and the lowest only 182 from 100 of alkaline gas; a difference which Dr. Henry cannot fully explain, but supposes it may arise from absorption of ammonia by mercury, and subsequent liberation by the shock.

The letter concludes with the mention of some experiments on the electrization of carburetted hydrogen, olefiant gas, carbonic oxide, and carbonic acid.

Carburetted hydrogen and olefiant gas were each expanded considerably in bulk; no carbonic acid was generated, but charcoal was deposited on the surface of the tube.

Carbonic acid was also partially decomposed by long electrization, and was converted into carbonic oxide and oxygen; and accordingly carbonic oxide underwent no change by the same operation.

Some Observations on the foregoing Paper of Dr. Henry. *By* Humphry Davy, *Esq.*

Mr. Davy, having lately had much occasion to pay particular attention to the electrization of ammonia during his researches on the decomposition of nitrogen, has observed various sources of error that may occur in the process, and may account for the difference between his results and those of Dr. Henry.

The first precaution that is to be taken, is to boil the mercury over which the decomposition is to be effected; next to prepare the gas in a separate vessel, and thence transfer it to the boiled mercury for electrization; for unless these precautions are taken, water contained in the mercury becomes the cause of that absorption of ammonia suspected by Dr. Henry, and by thus adding fresh ammonia during the process, occasions a fallacious result.

A second error may also be occasioned, when the mercury has not been boiled, by common air adhering to the sides of the tube.

In a late experiment, Mr. Davy obtained from 15 measures of ammonia 27 of permanent gases, consisting of 73 hydrogen and 27 nitrogen, agreeing so nearly with his original results of 74 to 26, that he conceives either of them to be more near the truth than Dr. Henry's estimate of 71½ and 28½.

With respect to Dr. Henry's attempt to prove the existence of oxygen in ammonia by the formation of water, Mr. Davy does not see much probability of success, as water existing in ammonia may elude any hygrometrical test. And although in his own electrization of ammonia the platina wires were tarnished, at the same time that the ammoniacal gas seemed to lose weight during decomposition, he

does not think the experiment conclusive, with regard to the existence of oxygen as an ingredient essential to the constitution of ammonia.

New analytical Researches on the Nature of certain Bodies, being an Appendix to the Bakerian *Lecture for* 1808. *By* Humphry Davy, *Esq. Sec. R.S. Prof. Chem. R.I.* [*Phil. Trans.* 1809, *p.* 450.]

Mr. Davy, having in the experiments described in the late Bakerian lecture, found that a quantity of nitrogen disappeared during the action of potassium on ammonia, and that it could not be made to resume its gaseous state but by the agency of oxygen in water, has been from that time much occupied in determining, with certainty, all the circumstances of the process; and though he cannot yet speak with precision as to the quantities, he thinks the general results decisive with respect to a decomposition of nitrogen having been effected.

When potassium is heated in ammoniacal gas, it becomes an olive-coloured fusible substance, losing all its metallic properties, a quantity of hydrogen is evolved, and ammonia disappears.

In the Bakerian lecture it was stated, that upon heating the olive-coloured substance a part of the ammonia is recovered; but when all moisture is carefully excluded, this quantity of ammonia does not amount to one tenth part of the quantity absorbed; and even this quantity appears to be owing to the almost unavoidable presence of moisture or oxygen.

In the present experiments, after taking all possible care to exclude moisture, since the glass of the vessels might possibly yield oxygen when in contact with potassium, a small tray of platina, containing the potassium intended to act upon the ammonia, was introduced into a retort containing the gas, and afterwards transferred expeditiously into a clean iron or platina tube made air tight, and furnished with a stop-cock.

In one experiment it was ascertained that 12½ cubic inches of ammonia are decomposed by nine grains of potassium, and evolve 8¼ of hydrogen.

In a second experiment, instead of reserving the gas for experiment, an equal quantity of potassium was used, and immediately transferred into an iron tube. The tube being then filled with hydrogen, and connected with a mercurial apparatus, was heated gradually. The quantity of gas collected previously to its acquiring a red heat, amounted to 9½ cubic inches, and four inches more were collected by raising the heat to whiteness. Of the former portion, about three fourths of an inch were ammonia; the remainder of the gas consisted of hydrogen and nitrogen, in the proportion of 8 to 3; so that the total quantity of hydrogen collected in this analysis, amounted to 16½ inches, and the nitrogen to three cubic inches. If the same quantity of ammonia had been decomposed by electricity, it would have yielded 15 of hydrogen, and 6 or 7 of nitrogen; so

that in the present experiment there was a loss of between three and four inches of nitrogen to be sought in the residuum which had been heated in the metallic tube. But when the tube was opened, nothing was found but potash that had been fused, and a small quantity of potassium sublimed into its upper part; so that in the place of the nitrogen nothing was found but oxygen contained in the potash, which, together with a small excess of hydrogen, may amount to the weight of the nitrogen lost.

In other experiments five grains of the olive-coloured ammoniacal pyrophorus, formed by potassium, yielded by mere heat, or by oxygen, three fourths of an inch of nitrogen; but by water they evolved one fifth of hydrogen, and $3\frac{1}{4}$ of ammonia, which by estimate contain $1\frac{1}{2}$ of nitrogen.

If the nitrogen is to be considered as converted into oxygen and hydrogen, it must be regarded as containing much more oxygen than water; and if we do not adopt this supposition, Mr. Davy considers the only alternative to be, that water is the ponderable matter which, under different modifications of electro-chemical existence, constitutes oxygen, hydrogen, nitrogen, and the nitrous compounds; but with respect to conclusions so important, and so little to be expected by any analytical chemist, it appears to Mr. Davy highly unphilosophical to decide in the present state of our knowledge.

The Croonian *Lecture. By* William Hyde Wollaston, *M.D. Sec. R.S.* Read November 16, 1809. [*Phil. Trans.* 1810, *p.* 1.]

Since the remarks, which the author has connected together on the present occasion, as tending, each of them, to promote the design of Dr. Croone, do not appear to bear any direct relation to each other, he divides the lecture into three distinct heads; the first of which contains an observation, and some experiments on the duration of muscular action. In the second he traces the origin of sea-sickness, from a mechanical cause deranging the circulation of the blood. In the third he offers a mechanical explanation of the advantages derived from riding, and the various modes of carriage exercise.

With regard to the duration of muscular action, the author is of opinion that each effort, apparently single, consists in reality of a great number of contractions, repeated at extremely short intervals. He infers the existence of these alternate motions, from a sound observed upon inserting the extremity of the finger into the ear. The sound resembles that of carriages at some distance passing rapidly over a pavement. The sound is not perceived when the force applied to stop the ear is not muscular, unless the action of some distant muscle be communicated through some medium capable of conveying its vibrations.

With a view to estimate the frequency of these vibrations, the author contrived to imitate them by rubbing a stick, regularly notched, and placing it in such a position, that the tremor was communicated to the ear along with the muscular vibrations. The results of such

trials was, that the vibratory alternations recur between twenty and thirty times in a second, but varying in number in proportion to the degree of force exerted by the muscle.

The utmost frequency which he has observed, he estimated at 35, and the lowest 15. But he considers the visible unsteadiness of an aged or infirm person, to arise from a less frequent repetition of the same motions.

In the second part of this lecture, which treats of sea-sickness, the author described an irregularity that he observed in his respiration, after having suffered some days from that affection, which appeared to be an involuntary effort of the constitution to relieve itself, by counteracting the effects of the motion of the ship.

In waking from a disturbed sleep, he remarked that each effort of inspiration was suspended for a time, and was then taken with a certain feeling of adaptation to some unknown motion of the sea. In reflecting afterwards upon this observation, it appeared to him that the act of inspiration might afford relief by means of its effect upon the circulation. For since, when the skull is trepanned, the effect of inspiration in withdrawing blood from the brain is manifested by the alternate heaving and subsidence of the brain, in alternate motion with the *opposite* states of the chest, the act of inspiration must tend to counteract any cause propelling blood to the head. And such a cause of pressure will manifestly occur in the descent of a ship by the subsidence of a wave on which it rests. When a person is standing erect upon deck, the motions of the column of blood contained in his vessels may be compared to those of quicksilver in a barometer. When the deck descends, the fluids no longer press with their whole weight against the force which supported them. The mercury continuing to be pressed with the whole weight of the atmosphere is seen to rise in the tube containing it, and so also the blood continuing to be pressed with the same elasticity of the vessels, which before supported its whole weight, is now driven upwards by the excess of force, and the most distressing sensation of sickness is then felt in consequence of its pressure upon the brain. But if an effort of inspiration be exerted at the same instant, it cannot but lessen this propensity, and have some effect in relieving the consequences.

An opposite effect is also noticed by the author to arise from motion in an opposite direction. For when a person rises very suddenly from an inclined position, and is at the same time, by previous fatigue, more than usually sensible of the consequences, he perceives a temporary sensation of faintness and giddiness, by partial abstraction of blood from the vessels of the brain; and may immediately relieve these symptoms by descending again suddenly to his former posture.

The explanation contained in the third part of the lecture, of the salutary effects of external or passive motion, is founded upon one necessary consequence which, he observes, must take place from mere mechanical agitation. Since the direction in which the circulation of the blood is carried forward in animal bodies, is given solely by the position of the valves that are to be found in the circulating

system, so also will a similar direction be given to the effects of external motion. Every motion tending to propel forward the blood, will hence assist the powers of the heart; but such as have a contrary tendency will be resisted by the interposition of the valves, and cannot occasion proportional obstruction to the regular progress of the blood; the heart is thus assisted in the work of restoring a system, which has recently struggled with some violent attack, or allowed, as it were, to rest from a labour to which it is no longer equal, when the powers of life are nearly exhausted by some lingering disorder.

It is conceived that all the other animal functions must participate in the relief thus afforded to so important an organ; and it is remarked, that even the powers of the mind itself, though most remote from our conception of material agents, are, in many persons, thus immediately affected, by the consequences of a merely mechanical operation.

The Bakerian *Lecture for* 1809. *On some new Electrochemical Researches, on various Objects, particularly the metallic Bodies, from the Alkalies, and Earths, and on some Combinations of Hydrogen. By* Humphry Davy, *Esq., Sec. R.S. F.R.S.E. M.R.I.A.* Read November 16, 1809. [*Phil. Trans.* 1810, *p.* 16.]

Mr. Davy having from the commencement of his electro-chemical researches, communicated the several steps of his progress to the Society, takes the present opportunity of reporting the results of his further inquiries under four principal heads. First, on the nature of the metals of the fixed alkalies. Second, on the nature of hydrogen and composition of ammonia. Thirdly, on the metals of the earths; and, Fourthly, he makes a comparison between the antiphlogistic doctrine, and a modified phlogistic hypothesis.

When Mr. Davy first communicated to us his discoveries of potassium and sodium, he adopted, as most probable, the antiphlogistic interpretation of the phenomena, and considered potassium and sodium as simple metallic bodies, of which potash and soda are the oxides. The same experiments have since been repeated by others with the same results, but the explanations given by different chemists have been various. The theory which has appeared most deserving the author's notice, and is more particularly controverted, is that of Messrs. Gay-Lussac and Thenard, who conceive these metals to be compounds of their respective alkalies with hydrogen; although in the interpretation of their own production of a metallic substance from boracic acid, they relapse again into the antiphlogistic doctrine, and suppose themselves to have effected a decomposition, by abstraction of oxygen from it.

Since the principal experiment on which Messrs. Gay-Lussac and Thenard rely, is that in which ammonia is acted upon by potash, Mr. Davy details a great number of modes in which he has varied the experiment with the utmost care to avoid moisture, which appears to

have misled the French chemists, and with the most scrupulous attention to the products.

When the experiment is conducted in tubes of iron, there is always a conversion of a portion of potassium into potash, a loss of nitrogen, and a production of hydrogen; but when platina or copper tubes are employed, the quantity of potassium remains the same, there is no loss of nitrogen, but there is a loss greater or less of hydrogen. The explanation suggested for this difference is, that an affinity of these metals for potassium may prevent its attracting oxygen from the ammonia.

For the decomposition of ammonia, sodium seems preferable to potassium, on account of the greater facility of employing it free from moisture; as the latter oxidates more rapidly at the surface, while transferring from one vessel to another, and more rapidly attracts moisture when oxidated.

Mr. Ritter founds the same opinion, that hydrogen is a constituent part of potassium and sodium, upon a singular circumstance that he has observed respecting tellurium; for he finds that this is the only metal by which potassium cannot be procured, when it is used as the conductor of voltaic electricity; and he ascribes the difference to the *affinity of tellurium for hydrogen* being *stronger than that of potash.*

From many experiments which Mr. Davy has made upon tellurium, and upon its alloys with potassium, he finds that tellurium unites with hydrogen as a solid hydruret of tellurium;—that it unites with a larger proportion of hydrogen as telluretted hydrogen, (a gas very analogous to sulphuretted hydrogen); that this gas combines with potash, forming a compound, corresponding to hydro-sulphuret of potash, and communicating to water a deep purple or claret colour.

After having thus ascertained the properties of tellurium, he found that when potash is acted upon by a very powerful battery, by means of a surface of tellurium at the negative pole, an alloy of tellurium and potassium is formed, which has the colour of nickel; when this alloy is thrown into water, the hydrogen, which in other instances is given off with effervescence, is not, in this case, extricated, but uniting with the tellurium, forms a hydro-telluret of potash, which communicates its purple colour to the water.

When a fusible alloy of potassium and tellurium was heated in ammoniacal gas, the permanent elastic fluid generated was nitrogen, not hydrogen, as is the case when potassium is employed alone; and this is considered by Mr. Davy as a proof, that in each instance the gas is derived from the ammonia and not from the metal, as the French chemists have supposed.

If the metals of potash and soda contained hydrogen, then water should be formed when they are burned. But when potassium is burned in close vessels in dry oxygen gas, or when sodium has been burned even in the open air, they do not yield hydrogen by being heated with filings of iron or of zinc, and they give no other indication of the presence of moisture.

But in order to compare potassium with its corresponding quantity

of potash in such state, as is allowed to be perfectly dry, according to the latest experiments of the most celebrated chemists, Mr. Davy converted eight grains of potassium into muriate of potash, by burning it in muriatic acid gas. Now, according to the experiments of Berthollet, recently published, eight grains of potash would make just twelve grains of muriate of potash, by the addition of four grains of acid. But the eight grains of potassium received an increase of six grains and a half, making fourteen grains and a half of dry muriate of potash, of which $4\frac{4}{10}$ths must be acid, according to Berthollet's estimation, and consequently there are $1\frac{8}{10}$ths of oxygen added to the potassium instead of hydrogen being extricated from it.

The endeavours of the author were next directed to obtaining more direct evidence of the composition or decomposition of nitrogen than he had hitherto done; and though his results have been mostly negative, he details a number of laborious and minute experiments, which he conceives may be of importance in settling various questions of doctrine that have been agitated.

Since nitrogen has been supposed to be produced during the decomposition of water by electricity, Mr. Davy has repeated that experiment by means of the powerful battery of the Royal Institution, kept in continual action for nearly two months, the product of gas being exploded about 340 times during the experiment; but the total quantity of permanent gas which remained, was not quite one quarter of a cubic inch, and this residuum was hydrogen, which may easily be referred to a slight oxidation of the wires of communication.

Other attempts were also made to form nitrogen or nitric acid from pure water, but all were unsuccessful. Wires of platina were fused by voltaic electricity in oxygen gas, saturated with moisture; with the hope, that at so high a temperature the water might combine with more oxygen; but this did not occur.

The vapour of water was passed over red-hot manganese, but no acid could thus be obtained, except by employing an unglazed porcelain tube, which was permeable to atmospheric air.

Since the formation of ammonia from pyrophori in various substances, appeared to indicate a formation of nitrogen, many such cases were examined; but it was found that the production of ammonia always depended upon the previous absorption of nitrogen by the charcoal present in such pyrophori.

With a view to decompose nitrogen, potassium was intensely heated in that gas by voltaic electricity, but without success. Phosphuret of lime was next substituted for potassium, but the nitrogen was not decomposed. Nitrogen was next mixed with oxymuriatic acid, and passed through a red-hot tube, without effecting any decomposition.

Notwithstanding such a want of conformation by any new processes, the original grounds for supposing oxygen present in ammonia remain. The amalgam produced from ammonia, which yields ammonia again by apparent oxidation, might lead to the inference

that both hydrogen and nitrogen are oxides; but from the action of potassium on ammonia, it would rather appear that nitrogen alone is in that instance decomposed.

But there is great difficulty in reasoning upon the precise nature of this amalgam, on account of the presence of water, which it is at least extremely difficult to avoid, as the amalgam cannot be formed in dry ammoniacal gas, nor by means of any dry compound of ammonia yet tried.

The driest amalgam is that formed by an alloy in which potassium is also present. In endeavouring to distil ammonium from this compound, there is always a partial regeneration of ammonia mixed with about one third hydrogen; and if the proportion of oxygen contained in ammonia be inferred from this product, it would appear to be forty-eight per cent.,—a result which agrees with the quantity which might be presumed to exist in ammonia, from the proportion in which it unites with acids. But if the proportion of hydrogen to ammonia thus evolved be less than that of one to two, the results will not accord; and then, says Mr. Davy, it may reasonably be supposed that hydrogen and nitrogen are both oxides, either of the same metal or of different metallic bases. But if, instead of endeavouring to accommodate our general antiphlogistic notions to the peculiar facts respecting ammonia, we endeavour to frame a phlogistic hypothesis to account for them, we must then suppose nitrogen to be a simple basis, which becomes alkaline with one dose of hydrogen; and metallic, by uniting with some greater proportion of the same element.

The author next details a variety of experiments, made on several of the earths, for the purpose of decomposing them. The metals of silica, alumina, and glucine, were obtained in alloy with iron; but it appeared that these metals could not be made either in direct combination with mercury, or as a triple alloy with mercury and potassium. By passing potassium, however, through the alkaline earths, lime and magnesia, and afterwards introducing mercury, solid triple amalgams were obtained. The triple amalgam from magnesia was easily deprived of its potassium by means of water; and it then appeared as a solid white metallic mass, which by long exposure to air was covered with a crust of magnesia. This section concludes with speculations on the probable quantity of oxygen contained in the earths, founded on Mr. Dalton's law of chemical union by simple particles, which appears to Mr. Davy more near the truth than that modification of it lately observed by M. Gay-Lussac.

The concluding section of the lecture contains some theoretic considerations on the nature of hydrogen in particular, and on the whole class of simple substances in general.

The fact of hydrogen uniting with tellurium and with sulphur into compounds apparently acid, it is observed, militate strongly against its being simple; and Mr. Davy inclines to consider it an oxide, of which the base exists in the amalgam of ammonium. Ammonia will then be the deutoxide of the same base, and nitrogen the tritoxide.

The class of pure inflammables will on this antiphlogistic hypo-

thesis be all metallic; some of them being as yet known only in combination, as those of sulphur, of phosphorus, and some others.

But a phlogistic hypothesis might also be framed, so as to account for all the phenomena with equal facility; the principal argument in favour of it being derived from the easy reduction of metals in presence of hydrogen. The ultimate predominance of one or the other hypothesis must, in the author's estimation, depend upon the nature of ammonia, of which the supposed elements do not present phenomena analogous to those of other simple bodies. Sulphur, for instance, whether combined with hydrogen or with oxygen, has acid properties; but nitrogen combined with one of those bodies is an alkali, and with the other an acid; so that in nitrate of ammonia, nitrogen is neutralized by nitrogen.

Mr. Davy finally adduces some new facts which accord with Mr. Dalton's hypothesis respecting the apparent union of ultimate atoms in a given relation *by weight*; and he also mentions some new instances in conformity to the observation of M. Gay-Lussac, that the different compounds of gaseous bodies are related in simple arithmetical proportions *by measure*.

The Case of a Man, who died in consequence of the Bite of a Rattle-snake; with an Account of the Effects produced by the Poison. By Everard Home, *Esq. F.R.S.* Read December 21, 1809. [*Phil. Trans.* 1810, *p.* 75.]

Thomas Soper, 26 years of age, was bitten by a rattlesnake on the 17th of October. The snake had refused to bite a ruler with which it had been teased, but as soon as the man introduced his hand into the cage for the purpose of taking out the ruler which had dropped in by accident, the snake seized his hand, and made two bites in succession, one on the thumb, and another on the fore-finger. The first person who saw him immediately after the bite supposed him to be intoxicated, and treated him accordingly; but though it is known that he had been drinking, Mr. Home is inclined to ascribe the incoherence of his language and behaviour to the effect of the poison.

In the course of half an hour a swelling had taken place on his hand, and half way up the fore-arm, with a great deal of pain. In an hour and half it reached the elbow; in two hours it extended half way up toward the shoulder, with much pain in the axilla. The man's answers were incoherent, his skin cold, his pulse 100 in a minute, and he complained of sickness at the stomach.

In the course of eight or nine hours the pain became extremely violent, the swelling very tense, so that the arteries could not be perceived with accuracy, and no pulse could be felt in any part of it. He was seized with fits of faintness every quarter of an hour.

The following morning his pulse was extremely feeble, and 132 in a minute. The swelling had not extended beyond the shoulder to the neck, but there was a fullness of that side down to the loins, with a mottled appearance from extravasation of blood. The arm conti-

nued cold, and painful when pressed, with several vesications near the elbow. The skin of the whole arm had a livid appearance, similar to what is met with in the dead body after putrefaction has begun to take place.

On the third day in the morning his pulse was scarcely perceptible; his extremities were cold; and the vesications were larger: but the swelling of the arm was diminished, and he had sensation in it down to the fingers. On the fourth, fifth, sixth, seventh, eighth, and ninth days the swelling continued subsiding, but more painful; and his pulse rising in strength, and diminishing in frequency. But on the tenth, his pulse had increased, and the arm was again swelled and inflamed. On the twelfth a large slough began to separate from the inside of the arm, below the axilla; and on the following day a large abscess had formed on the outside of the elbow, and discharged a quantity of reddish matter, with slough of cellular membrane floating in it.

On the sixteenth, it appeared that mortification had taken place on the skin near the axilla; and on the eighteenth he died.

The body was examined after death, and the appearances on dissection are described by Mr. Home. The most remarkable circumstance observed, was the entire separation of the skin from the muscles of the arm, with the exception of two or three, to which it still adhered imperfectly by a dark-coloured cellular membrane. The fluid in the pericardium, and blood in the aorta, had an unusual frothy appearance.

In addition to the foregoing case, Mr. Home adds an account which had been sent from India to the late Dr. Russel, of a Sepoy, 60 years of age, who had been bitten by a Cobra di Capello, on the back part of the hand, in whom the symptoms were extremely similar to those which occurred in the case above described; pain, faintness, sickness, with a quick and feeble pulse, swelling, discolouration, vesication, abscess, a copious dark and fœtid discharge, continuing for about a week; after which the man gradually recovered, excepting that one finger remained permanently extended.

Mr. Home also gives an account of two experiments which he made in the West Indies, on a couple of rats; the first of which, after being bitten by a venomous snake, died in one minute after the bite; and it is remarked, that the cellular membrane under the skin of the side bitten, was entirely destroyed.

The second rat, which was bitten one quarter of an hour afterwards by the same snake, was not so soon affected by the bite; and bit the snake so violently in the neck, as to kill it in ten minutes. The rat itself continued lively for nearly six hours, and then died.

An Analysis of several Varieties of British and Foreign Salt (Muriate of Soda), with a view to explain their Fitness for different economical Purposes. By William Henry, *M.D. F.R.S., Vice-Pres. of the Literary and Philosophical Society, and Physician to the Infirmary, at Manchester.* Read January 25, 1810. [*Phil. Trans.* 1810, *p.* 89.]

An opinion having for some time prevailed, both in this and in other countries, to the prejudice of British salt as a preserver of animal food, and large sums of money being annually paid to foreign nations, in conformity to this opinion, for the supply of an article which Great Britain possesses beyond almost any other country in Europe, Dr. Henry has been induced to undertake the present inquiry, for the purpose of ascertaining whether the preference of foreign salt be founded on accurate experiments, or merely a matter of prejudice; and in the former case, whether any chemical difference could be discovered that would explain that superiority.

The subject is divided into three parts: in the first of which are contained, general observations on the different kinds of salt manufactured, and the modes of preparing them. The second division contains the general statement of the results of the experiments, and conclusions deducible from them. But since a long detail of analytical processes must be uninteresting to very many persons, to whom the results will be acceptable, the description of the method employed by Dr. Henry, in his analysis, is reserved for the last section, in which (as the author observes) are not to be expected any novelties in science, but merely a careful selection and employment of known processes.

The principal sources of the salt manufactured in this country are: the rock salt of Cheshire; the brine springs, which are also mostly in the same district; and sea water, which cannot be evaporated artificially with profit, excepting where fuel is cheap.

Concerning the preparation of salt in Cheshire, Dr. Henry extracts a brief statement from an excellent history given by Mr. Henry Holland, in the agricultural report of the county of Chester; and he notices four varieties.

First. That which is called *stoved* or *lump* salt. Second. Called *common* salt. Third. Large-grained flaky salt. Fourth. Fishery salt;—the difference among these depending principally on the degree of heat used for evaporation of the brine.

The stoved salt is prepared by rapid evaporation at a boiling heat of 226° Fahrenheit, and it obtains its name from being subsequently dried in stoves after being well drained.

For making common salt, the brine is evaporated at a temperature between 160° and 170°; and as the salt is consequently formed in somewhat larger grains, it is merely drained, and does not require to be dried in stoves as the former. The name of the third, or large-grained flaky salt, implies a slower process of crystallization, and it is conducted at a temperature of 130 or 140 degrees.

The last, or fishery salt, which is the largest, is prepared at a heat

of 100 or 110 degrees. This process is so slow as to last seven or eight days, instead of eight or ten hours, which is the shortest time employed in the first mode of evaporating the same quantity of brine, and accordingly the salt forms in large cubical crystals, proper to the muriate of soda.

Of these varieties, the stoved salt is that which is usually employed for domestic purposes. The common salt is consumed principally in the salting of provisions that are not intended for sea-voyages. But for this last purpose, the large-grained or fishery salt is peculiarly fitted.

With respect to the preparation of salt from sea-water, the author takes notice of the process employed on the coasts of Scotland, both east and west, where, from the cheapness of fuel, artificial heat alone is used; and of the salt-works at Lymington in Hampshire, where advantage is taken of a milder climate for removing five sixths of the water by spontaneous evaporation, previous to its admission into the boilers.

Since the evaporation in each of these works is conducted rapidly during the formation of the salt, it generally resembles the stoved salt of Cheshire; but in consequence of the heat being slackened during Sundays, a larger kind is then formed, and it is termed Sunday salt. At Lymington, there are also formed, by drippings from the salt during its drainage, large stalactical masses, termed salt-cats, weighing sixty or eighty pounds each; but these do not exceed $\frac{1}{100}$th part of the salt prepared at Lymington.

Of the several salts above described, the large-grained fishery salt is that which most resembles the foreign bay-salt in appearance; and in fact (says the author,) a large proportion of what is sold in London as bay-salt, and esteemed as of foreign manufacture, is this Cheshire salt.

Dr. Henry next gives a table of the results of his chemical experiments on eleven varieties of salt; and in this it appears, that the *Lymington cat* is that which contains the smallest quantity of impurity, and the Lymington or Scotch common salt the largest quantity; the first amounting to only 12 parts in 1000, and that of the last to 64.

But it appears that all the kinds of Cheshire salt are nearly equal to the *Lymington cat* in purity, and perhaps superior in respect to the *quality* of the extraneous matter, which varies in different kinds from $13\frac{1}{2}$ to $17\frac{1}{2}$ in the thousand. The foreign bay-salts, on the contrary, have as much as from 35 to 40 parts of impurity. Of these about 10 parts are insoluble, and consist chiefly of argillaceous earth, coloured by oxide of iron. The native rock salt of Cheshire also contains as much or more of insoluble impurity, which is chiefly a marly earth, with some sulphate of lime. The earthy muriates of lime and magnesia abound most in salt which is prepared by rapid evaporation of sea water.

Since common salt contains extremely little water of crystalliza-

tion, it is pretty evident that the earthy muriates discovered in the analysis of sea salt are derived from the portion of the mother water which adheres to the salt after being drained; and accordingly, those salts prepared from sea water that are smallest grained, and consequently have the largest proportion of interstice, are debased by the largest proportional quantity of this species of impurity. But of this impurity the Cheshire salts are nearly free, as they do not contain one part in 1000 of earthy muriates; and indeed it is scarcely possible that any portion of Cheshire prepared salt can contain more, since the rock itself does not contain more than 5 in 1000; while on the contrary, in sea water, the earthy muriates amount to no less than $\frac{2}{1000}$ths of the entire quantity of salt contained. Dr. Henry pays particular attention to these muriates, because the propensity of common salt to deliquesce by attracting moisture from the atmosphere, depends in great measure (though not entirely) on the presence of these deliquescent compounds.

Since in the analysis of salts nominally the same, great difference often occurred even in examination by the same process, Dr. Henry endeavoured to trace the origin of this disagreement of his results. And, as he conceived it might arise from the different degrees of purity of the liquor in different stages of its evaporation, he procured three samples of common salt, of which one was taken from the boiler two hours after the first application of heat; the second at the end of four hours; and the third at the end of six hours: and he found

The first to contain 16 parts of sulphate of lime in 1000;

The second to contain 11; and,

The third only $3\frac{1}{2}$.

But on the contrary, when the impurities are of a different species, and are highly soluble, these will be found to abound most in the salt last drawn, on account of the large proportion they then bear to the aggregate contents of the mother liquor.

The author also ascertained the quantities of water contained in the several varieties of salt; but this he found to be very small, and not constant in any one, appearing rather as an accidental than a necessary ingredient in any of them.

Since the differences of chemical composition discoverable by experiment are not sufficient to account for those properties which are imputed to the several varieties of muriate of soda, the author is of opinion they must depend upon some mechanical property; and the most obvious are the magnitude of the crystals, and their degree of compactness or hardness, which must each retard the process of solution; since a given weight of the salt will expose less surface for solution, even from mere magnitude of its particles; and hence will remain more permanently between the different layers of provisions, and furnish a constant supply of saturated brine during the gradual exudation of the fluids originally contained.

For the purpose of estimating the compactness of several different varieties of salt, Dr. Henry took some pains to measure their specific

gravities, by putting equal weights successively into the same vessel, and again weighing it after filling the interstices with a saturated solution of salt.

The specific gravity of rock salt was found, thus, to be..	2125
That of the same broken into small fragments	2112
That of stoved salt was also........................	2112
Common salt	2084
Fishery salt..	1909
St. Ubes, as a specimen of bay-salt	1932

The difference between the large-grained fishery salt and the bay-salt of foreign manufacture is so inconsiderable, that although the superiority of the former in chemical purity may not be considered as of any advantage for economical purposes, yet in its mechanical qualities it cannot be said to be inferior in a degree that can be prejudicial.

The methods of analysis employed by the author in this inquiry are next described. The salt to be examined was first dried at a given temperature of 180°. The earthy muriates were then separated by alcohol, and their aggregate weight ascertained after evaporation of the alcohol. An aliquot part was next dissolved, and the lime precipitated first by carbonate of ammonia, after which the magnesia was separated by phosphate of soda, as a triple ammoniacal phosphate of magnesia. A previous trial having shown that 100 grains of dry muriate of magnesia would give 151 of the triple phosphate, the quantity of muriate of magnesia was inferred from this latter precipitate, and the difference between that and the aggregate weight of the two muriates was considered as muriate of lime. Sometimes the estimation was formed in a different way, by superoxalate of potash, which was found to occasion a precipitate of 116 grains from 100 dry muriate of lime; and thence, as before, the weights of each might be inferred.

For estimating the earthy sulphates, the quantity of original salt that remained after affusion of alcohol was dissolved by long boiling in water; the earths were precipitated as carbonates by carbonate of soda. The sulphuric acid was separated by muriate of barytes, and thence estimated. The earths were then re-dissolved in sulphuric acid, dried, and their weight ascertained. Of these sulphates, the more soluble part was dissolved in a small quantity of warm water, and the magnesia precipitated, as in the former case, as a triple phosphate of magnesia.

It was found that 100 grains of this precipitate indicate 111 of crystallized sulphate of magnesia; and hence the respective quantities of the two sulphates was known: but since it was possible that some proportion of alkaline sulphates might be also present, some collateral experiments were necessary for the purpose of ascertaining whether the sulphuric acid obtained above by muriate of barytes, corresponded with that which would be contained in the mere quantities of sulphate of magnesia and sulphate of lime discovered

to be present. The quantity of sulphate of barytes produced from 100 grains of sulphate of lime was accordingly ascertained, and found to be 175·9; and 100 grains of crystallized sulphate of magnesia were found to give 112 of sulphate of barytes. And since the aggregate quantity of sulphuric acid obtained from any quantity of salt examined was found to agree with the above proportions, it was inferred that no alkaline sulphate was present in any of the varieties of muriate of soda, whether of English or foreign manufacture.

In addition to the author's account of the methods pursued in his analyses, he also mentions various objects of inquiry respecting the preparation of salt, which may be interesting to chemical readers:—such as the specific gravity of the original brine of Cheshire, and its original contents; the specific gravity of mother liquors, and their ultimate contents; the clearings of brine, which are raked out as soon as the salt begins to granulate; the pan-scale, that forms as a hard crust, attached to the pan in which the brine is evaporated; and the varieties in this scale, under different circumstances.

The difference between sea-water and the brine from salt-springs is also stated, and the extreme difference also of the residua obtained from the respective mother liquors, especially in respect to muriate of magnesia; since the mother liquors of Cheshire contain only 35 parts in 1000, while that of the other amounts to 874; the mere refuse of the Cheshire processes being nearly equal in purity to some kinds of salt prepared from sea-water.

Description of an extraordinary Human Fœtus. In a Letter from Mr. Benjamin Gibson, *Surgeon, to* H. Leigh Thomas, *Esq. F.R.S.* Read February 8, 1810. [*Phil. Trans.* 1810, *p.* 123.]

Although instances of human bodies nearly entire, united side by side, or back to back, or otherwise, are by no means rare in the collections of anatomists; and although such a conjunction is generally not connected with any peculiarity in the organs which compose them, and lead the physiologist to anticipate nothing curious in their internal configuration;—yet, where some parts are found double, and others single, the resources of nature become apparent in adjusting parts which have naturally no connexion. Such is the instance here described; and it appears peculiarly interesting, from the consideration, that the system of deviation was apparently compatible with life; for if the difficulty of the birth had not proved almost immediately fatal, the complexity of the structure would probably have formed no impediment to its existence.

This curious production had two heads, placed side by side, united to one body, with two legs and two arms. The countenance of the one appeared to the author to be male, and of the other female; and the conformation of the organs of generation, which partook of both sexes, confirmed that persuasion.

The trunk, though appearing as one body, was broader than natu-

ral; and it was evident, even from external examination, that there were two spines corresponding to the two heads. These were found to terminate in a double os sacrum, tipped with two ossa coccygis; there were consequently two vertebral canals, independent of each other. The chest in front was nearly as usual, with the common number of ribs on each side; but there were also, in the middle of the back, short bones, which must be considered as ribs connecting the two spines to each other.

The chest was divided by its mediastinum into two cavities. In the right was one lung connected with the right head, and in the left another belonging to the left head, without any mutual communication. On inspecting the vascular system, there were found to be two hearts; that on the left, and corresponding with the male head, was much the larger of the two; but they both had the usual number of auricles, and were inclosed in separate pericardia.

The principal difference in the distribution of their vessels was observable in the veins; as the vena cava superior was wanting on the right side, and the right heart received its blood by the vena cava inferior alone; while that on the left had its principal supply by a large vena cava superior, which brought the blood from the jugulars of the right head and right subclavian, as well as from those of its own side. But, on the contrary, the vena cava inferior was comparatively very small.

With respect to the arteries, the difference was not considerable, as they each sent off their respective aorta and pulmonary artery, and each supplied their respective head by two carotids, and their corresponding arm by one axillary artery; after which the two arches united into one trunk as descending aorta.

Little irregularity was observable in the cavity of the common abdomen, the stomach of which was connected with an œsophagus from the left head; but in a second abdominal cavity, formed by an oblique position of the diaphragm, there was also a smaller stomach belonging to the right head, with a spleen, pancreas, and omentum; but the duodenum from this stomach soon communicated with that from the left side, and the remainder of the intestinal canal was continued onwards, with little deviation from the natural structure.

The organs of generation were very remarkable, as they distinctly partook of both sexes; for though, in the external parts, the character of the male was predominant, with two testes in the act of passing into the scrotum, yet, in some respects, certain deviations belonging to the female were even here apparent; and, upon internal examination, an uterus was discovered imbedded in the posterior part of the bladder, from which Fallopian tubes were given off at each side near its upper part.

The author next examined carefully the distribution of the nerves, and found, in general, extremely little connexion between them, excepting at the semilunar ganglion, which was composed of nerves derived equally from both sides.

Each brain, for instance, gave origin to an eighth pair of nerves,

that supplied, as usual, the heart, the lungs, and the stomach belonging to its own side of the body, and then gave branches, as usual, to the semilunar ganglion.

A pair of intercostals also proceeded from each head. The left intercostal from one, and the right intercostal from the other, proceeded as usual; but those which passed down between the two spines, and adjacent to each other, neither gave off nor received communicating branches, and did not form the usual ganglia, for want of all the cervical, dorsal, and lumbar nerves excepting the first cervical. The other nerves were in general distributed as if the body had had but one spine: thus the nerves of the right arm were supplied from the right side of the right spinal marrow, and those of the left arm came from the left side of the left spinal marrow.

The author next proceeds to such remarks as naturally arise from the contemplation of so singular a production. And the first observation is, that although there were here combined, in the same system, duplicates of several parts, yet there appeared, upon inspection, to be no reason why the functions of life might not have been properly performed. An apparent uniformity prevailed throughout, without abruptness, in the transition from the double to the single structure. Two stomachs terminated in one track of intestines, somewhat more capacious than usual. Although the vascular system was furnished with two hearts, the principal vessels were arranged and united together, so that no impediment seemed to exist to the circulation of the blood. For regulating the supply of blood to each heart, a communication was established between the vena cava superior of one heart, and the vena cava inferior of the other.

The nervous system presents also a wide field for conjecture. The function of the two brains appeared to be as independent of each other as those of any two individuals of the same family; and the reasoning faculties, it is presumed, might have been very opposite, excepting so far as they would have been influenced by the same habits, and by the same course of education; and their sensual inclinations (if any such existed) would probably be very different.

As the vital organs conspired together in carrying on the same process of life, so did the nerves of these involuntary parts unite for the same purpose, though originating from different heads. But, on the contrary, those nerves which supply the voluntary organs were as completely unconnected with each other as if they had belonged to different bodies. So that although in all involuntary and indispensable functions there would probably have been the most perfect harmony, considerable inconvenience might have arisen, where locomotion was concerned, if the two heads, thus inseparably coupled, had not had the same inclination.

Observations on the Effects of Magnesia, in preventing an increased Formation of Uric Acid; with some Remarks on the Composition of the Urine. Communicated by Mr. William T. Brande, *F.R.S. to the Society for the Improvement of Animal Chemistry, and by them to the Royal Society.* Read February 22, 1810. [*Phil. Trans.* 1810, *p.* 136.]

Mr. Home's inquiries into the functions of the stomach, and his discovery of liquids passing directly from the cardiac portion into the circulation of the blood, occasioned him to consider the prevention of calculous complaints, by correcting the generation of acid in the stomach, and consequent secretion of uric acid.

Since magnesia was better adapted to the mere correction of acidity than alkalies, as it could not be absorbed till it had been previously dissolved, Mr. Home was desirous of examining its effects in preventing the generation of uric acid, and requested Mr. Brande's assistance for that purpose.

After several previous trials, in which it appeared that an excessive secretion of uric acid was corrected by magnesia more than by a liberal use of alkalies, it was afterwards tried in various cases of confirmed calculus; and four cases are selected as instances of the principal varieties of that disorder, from many others in which magnesia was tried.

The first was that of a gentleman of 60, who had passed small calculi of uric acid.

He had first taken subcarbonate of soda in water highly impregnated with carbonic acid, to the quantity of nine drachms in a day, but without any apparent effect on the secretion and formation of uric concretions.

He then took, in the same manner, subcarbonate of potash, dissolved in water impregnated with carbonic acid, to the quantity of three drachms every day; but though the deposit of sand from the urine was in some degree diminished, yet small calculi continued occasionally to be voided.

On the contrary, by taking as much as fifteen grains of magnesia three times a day, the quantity of uric acid was diminished in quantity; and after three weeks was only occasionally perceived in his urine.

The second case is that of a gentleman 40 years of age, who for four years preceding had occasionally passed much red sand, and once a small calculus.

Subcarbonate of soda was first given him in water highly impregnated with carbonic acid, which had the effect of diminishing the secretion of uric acid, but not of preventing occasional severe attacks after irregularities in his diet.

Magnesia was next directed, to the quantity of twenty grains every night and morning; and during six weeks' continuance of this preventive, he had no return of his complaint, and no superabundance of uric acid in the urine.

The third was a gentleman of 43, who had passed three small calculi, and whose urine had for a short time been constantly turbid, and occasionally deposited red sand.

By the use of soda-water, these symptoms were diminished, but returned in some degree even during the continued use of it; and his urine became also loaded with mucus.

By taking twenty grains of magnesia every night, the uric acid diminished in quantity, but did not disappear entirely, even by the continued use of magnesia for three weeks in the same quantity. It was then repeated every night and morning for a month, and succeeded in restoring the urine to a perfectly healthy state. Upon a return of the disorder, he had recourse again to the magnesia, with the same effect.

The fourth patient was subject to gout, and occasionally voided abundance of red sand, consisting of uric acid. He was subject to heart-burn, and to other pains attendant upon weakness of stomach, for which he had been in the habit of taking tincture of bark and other spirituous medicines. He had also tried the use of alkalies, but could not continue them on account of the unpleasant sensation they occasioned in his stomach.

Magnesia was accordingly given, at first three times a day, and afterwards in a quantity of twenty grains twice every day; and it had the effect of lessening the disposition to form uric acid, and appeared also at least to suspend the attacks of gout for a greater length of time than he had been accustomed to.

Comparative trials were afterwards made of the effects of the alkalies and of magnesia upon healthy urine.

Two drachms of subcarbonate of soda seemed to Mr. Brande to produce its full effect upon the urine in a quarter of an hour after it had been taken, occasioning a precipitation of the phosphates of lime and magnesia, and giving other indications of its presence, by restoring the blue colour to litmus-paper.

When supersaturated carbonate of soda was taken, the precipitation of the phosphates was less distinct and less rapid, as they remained dissolved for some time by excess of carbonic acid in the urine, and then began to appear as a pellicle at the surface by gradual escape of the carbonic acid in the form of gas.

In experiments with potash, the results were the same as when soda was employed.

Magnesia had also the same effect of occasioning a precipitation of the earthy phosphates; but on account of its insolubility, a greater length of time was required to produce the effect.

Lime-water also required as much as five hours to produce a sensible precipitation; and even then it was not nearly so distinct as from the alkalies.

Since the effects of soda or potash were altered by the presence of carbonic acid, the acid itself was tried alone, by taking twelve ounces of water highly impregnated with carbonic acid; and as it evidently passed off by the kidneys, and appeared in the urine of a healthy

person, it was afterwards tried in one who was subject to calculus, consisting of the triple phosphate of magnesia. Though his stomach did not admit the use of stronger acids, the carbonic acid proved highly grateful; and by examination of his urine, it appeared that the phosphates, which before were voided as a sediment of white sand, were now passed only in a state of complete solution, by means of the redundant acid.

Supplement to the First and Second Part of the Paper of Experiments for Investigating the Cause of Coloured Concentric Rings between Object-glasses, and other Appearances of a similar Nature. By William Herschel, *LL.D. F.R.S.* Read March 15, 1810. [*Phil. Trans.* 1810, *p.* 149.]

The Supplement now offered to the Society, is intended to clear up certain points which have been represented to the author as obscure or doubtful in his former communications, and at the same time to connect more intimately the prismatic experiments of the second paper with those made upon convex glasses, and described in the author's first paper on the subject.

Since Dr. Herschel has heard the originality of his observation of the red bow called in question, upon the ground that a red bow had been observed by Sir Isaac Newton, which is merely the converse of the blue bow (the change of colour being dependent upon the direction in which the light is received upon the prism), Dr. Herschel first endeavours to answer the objection, and reminds us that in his former observations the angular breadth and elevation of the two bows are different; but those of the Newtonian blue and red bows are said to be, and are, necessarily equal. In the Newtonian experiment also, the same beam of light is made to exhibit both phenomena, being received upon two right-angled prisms, applied base to base, so that one portion of the light is reflected upwards, as a blue bow from the under surface of the first prism; and the remainder, by transmission, through the second prism, appears as a red bow to an eye beneath. But in Dr. Herschel's experiment, the same prism is made to exhibit, to an eye in the same situation, the red bow as well as the blue, by means of light transmitted in an opposite direction through the under surface of the prism, without any occasion for a second prism, which (as Dr. Herschel observes) is necessary in the Newtonian method of conducting the experiment.

The next objection replied to by Dr. Herschel, regards the streaks that may be seen adjacent to the bows when a second surface is applied to that side of a prism at which a critical separation of the colours takes place. It has been said that streaks parallel to the bows, though not dependent on critical separation, will in that situation be seen most easily and most distinctly, because the visual ray, under those circumstances, passes with the greatest obliquity between the surfaces.

To this objection Dr. Herschel replies, that these streaks not only

can be seen most easily and most distinctly in the place where the bows are, but they absolutely cannot be seen anywhere else.

The parallelism also of the streaks to the bows, in the author's estimation, proves that the same cause which determines the direction of the bow, must determine that of the streaks, and thus establishes their dependence on critical separation.

Dr. Herschel also contends, that streaks of different colours could not be produced by a plate of air, of uniform thinness, between plain surfaces, and that the prevalence of a blue colour in the streaks belonging to the blue bow, and of the converse in those belonging to the red bow, prove their dependence on critical separation.

Since it has been conceived by other persons, that by means of a plate of air, having the form of an extremely thin wedge, straight bands of colour would be produced between plain surfaces slightly inclined to each other; and as an experiment in support of this opinion had been shown to the author, he gives his own explanation of the fact: and he ascribes the production of the colours to distortion of the surfaces, because a degree of force was in that instance employed for the purpose of producing the requisite contact at one extremity of the glass. And since in other experiments, made with perfectly plain surfaces, where no pressure was employed no streaks could be seen, Dr. Herrchel concludes that when streaks are seen, the surfaces employed are either not plain in their general extent, or are terminated by some inconceivably small curvature at the edges in contact.

It has, in the next place, been observed to the author, that in the enlarged figure which he has given in his last paper to illustrate the streaks, the vacancies observable correspond with, and depend upon, the assumed intervals between the rays, which in that figure are represented as originally separated by blank spaces.

Dr. Herschel admits that there is some plausibility in this objection, but informs us, that the supposed force of it is founded on a misconception of the figure, which is not designed to represent the visible arrangement and colours of the streaks, which can only be deduced from their mixture at the place where they enter the eye; but he declines a thorough investigation of this point, because it would really be an endless undertaking.

One section of the present communication is devoted to the consideration of the breadth of the streaks compared to that of the bows, and the cause why they must take up a broader space than the bows from which they are derived; because it has been remarked, that this circumstance precludes the possibility of accounting for them by critical separation. But although this remark may at first view appear to be justified, it must be remembered (says the author) that the modifying power of the surfaces is added to the principle of the critical separation. The modification specifically named by the author, is that of reflection by the plain surface held under the prism, which, in the first instance, magnified the extent to 2½ times the breadth of the bow; and if the reflection be repeated any number of times be-

tween the two adjacent surfaces, it may increase the extent in any greater proportion.

The last objection to which the author replies, relates to those positions in which *rings of colours*, and other *similar phenomena*, are seen, but in which the colours produced by critical separation could not reach the eye. For instance, rings and bands of colour, which arise from contact at the under surface of a plate of glass terminated by parallel planes, are seen through the upper surface, although colours separated by critical reflection or intermission, evidently could not come to the eye under these circumstances. But Dr. Herschel reminds us, that he does not affirm critical separation to be the sole cause of the rings produced by contact of a plane and sphere, but that it only furnishes the colours, which are afterwards modified by the subjacent spherical surface; and next proceeds to several sets of experiments, which he considers decisive in support of the validity of his theory, in reply to this objection.

In the first set of experiments a series of prisms, of different forms, are successively placed within their bases, resting upon a spherical metallic surface.

When a right-angled prism was placed in this situation, and the eye was gradually raised from a level with its base, no colours were seen till it arrived at the elevation necessary for critical separation. At this point the blue bow became visible, and rings began to be perceived at the same time. When the eye was lifted gradually higher and higher, till it arrived opposite to the vertex of the prism, the rings continued visible, without interruption, notwithstanding successive changes which occurred in their colours and size: and even when the eye was carried beyond the vertical position, the same rings continued visible, so as to be seen, upon the whole, through a range of at least 77°.

Instead of the right-angled prism, having a refracting angle of 45° on each side, Dr. Herschel afterwards substituted prisms with their vertical angles successively more obtuse, and with equal refracting angles on each side, first of 30°, then of 25°, then of 20°, and lastly of 9° on each side.

In all these instances the phenomena were similar; but the range of visibility increased in proportion as the refracting angle was smaller, so that in the last instance the range within which the rings were visible from each surface, exceeded 138°. And hence might be inferred the still greater extent, in case of plain glass, which may be looked upon as a prism with a vanishing refracting angle.

These experiments, in Dr. Herschel's estimation, establish the modifying power of spherical surfaces, whereby they render colours that have been entirely separated, visible in every direction.

In the next set of experiments, the author substitutes a cylindrical surface for that which in the former set was spherical; and by a similar series of prisms, successively more and more obtuse at their vertical angles, the coloured streaks, which in this case appeared instead of rings, were rendered visible to greater and greater extent, till with a plain glass they were seen as far as 170°.

In a third set of experiments, conducted in the same manner, the under surface brought into contact with the prisms, consisted of mica, rendered nearly cylindrical by being bent over a cylindrical surface.

From the irregularity in the form of the mica, that of the colours was also irregular; but they served to show the increase of extent to which such appearances may be rendered visible by corresponding change of the angle of the prism.

Dr. Herschel is consequently of opinion, that any one who could object to the admission of critical separation as the cause of the phenomena under consideration, cannot have paid sufficient attention to the modifying power of the subjacent reflecting surface, which is so essential to their formation.

If any one is disposed to assume that the rings must arise from some other cause than critical separation, unless it can be shown how rays critically separated can reach the eye, the author thinks it is not to be expected that *he* should trace them through a most intricate complication of reflections from curve to curve, when it has been shown, in the second part of this paper, that even with streaks produced by contact of two plain surfaces, it would be an endless attempt to follow them. He accordingly thinks it sufficient to have proved, to his own satisfaction, two essential points; first, that colours separated critically may be formed into rings, when modification will increase the field of visibility to any extent beyond the limits of critical separation.

Enough (says the author) has been said to prove that the phenomena of coloured rings, and other phenomena that have been ascribed to certain fits of easy reflection and easy transmission, admit of the most satisfactory explanation, by substituting the solid principle of the critical separation of the different colours, in the room of these fits.

On the Parts of Trees primarily impaired by Age. In a Letter from Thomas Andrew Knight, *Esq. F.R.S. to the Right Hon. Sir* Joseph Banks, *Bart. K.B. P.R.S.* Read March 22, 1810. [*Phil. Trans.* 1810, *p.* 178.]

In the first communication which Mr. Knight made to the Society in the year 1795, he showed that the period to which the existence of any one variety of fruit could be prolonged by grafting, was limited; and that any portion detached from an old tree, and transplanted upon a young stock, was not thereby restored to what can, with propriety, be called a young tree.

Mr. Knight's endeavours have, since that time, been directed toward ascertaining which of the several organs it is that first fails in the performance of its proper office in consequence of age, and the result of his experiments forms the subject of the present letter.

In the prosecution of these inquiries Mr. Knight bears constantly in mind the analogy that subsists, in many respects, between the organs of animals and those of vegetables; for though it may not be in his power to avail himself of any assistance to be derived from

such considerations, it is not improbable that, on the contrary, some new light may be thrown upon the functions of the animal economy by investigations respecting those of vegetables, where the necessary experiments may be repeated any number of times, and where the influence of efficient or defective organs may be observed with the most deliberate attention.

The parts separately noticed on the present occasion, are the roots, the stem, and the leaves. The roots and leaves have been compared by all naturalists, both ancient and modern, to the intestines and lungs of animals. The analogy also, between the sap of vegetables and the blood of animals, is very obvious; and the circulation of sap in the former, as far as is necessary to, or consistent with, their state of existence, is very satisfactorily established by the experiments formerly communicated to us by Mr. Knight, in addition to those made by other naturalists.

With respect to the roots, no experiments appeared wanting to determine that no defect in the action of this organ occurs from age, and consequently that the debility and diseases of old varieties of fruit were not derived from this source. The duration of roots, in old coppices, that are felled at stated periods, appeared to the author sufficient to establish that the quantity of produce is not diminished by *age* of the roots. The inability also, of a seedling stock to give the character of youth to an inserted bud or graft, seemed to prove how little is effected by undoubted *youth* of the root.

Mr. Knight ascertained, however, by an experiment of an opposite nature, that the *stock* may be affected by the *graft*. By planting cuttings of some very old varieties of apple, he obtained stocks which would soon have manifested the usual appearances of age. At the end of two years these were grafted, at about two inches from the ground, with new and luxuriant varieties; and at the end of five years the roots were examined, and were found to contain ten times as much alburnum as they would probably have contained if they had not been grafted; and they were also wholly free from disease.

Another kind of experiment was next made upon the effect of grafting young wood upon old, the old having first been grafted upon a young stock, in a situation where it would not have survived the second or third year. But when a portion of an old golden pippin was thus included between two portions of a crab, the wood was found to grow just as well, and to be just as healthy as the stock and branches.

In other experiments the author tried the effect of placing young grafts upon old ones, that had long since become cankered. The old ones being cut off at the distance of a foot from their original junction, he regrafted them with new and healthy varieties, and he found that they became, in consequence, perfectly freed from every appearance of canker.

The author having thus ascertained that the debilities of old varieties of fruit-trees did not originate in any defective action of the

bark or alburnum of either the stem or the root, he proceeded to investigate the states of the leaf, and of the succulent annual shoot. With this view various grafts of the golden pippin, which were known to be liable to decay, were inoculated with buds of new varieties; and in the ensuing winter their own *natural* buds were removed, and those that had been inserted were alone allowed to remain. As soon as the leaves of these began to appear, every symptom of disease was removed; and each part of the branch of the golden pippin thus regenerated, appeared to perform its office as well as the wood and bark of the seedling stock could have done without this intermediate graft of old materials.

Since the vigour of youth, or debility and diseases of old age, appeared thus to depend on the quality of the leaf through which the sap of plants circulates, in the same manner as the blood of animals does through their lungs, Mr. Knight considers the consequence of defective leaves, according to his former views of the functions they perform, of preparing and assimilating the sap transmitted through them; and he observes, that the deficiency of power in the leaves is (as might be expected) most apparent where the redundancy of sap is the greatest; for he finds that the grafts of old varieties are most diseased in rich soils, or when they are applied to vigorous stocks; and the defects appear to arise from an accumulation of fluid in the extreme branches and annual shoots, beyond what can circulate with effect through the imperfect leaves that are produced by extremities debilitated by age.

In support of this opinion, of an essential difference between the leaves of young and of old varieties, Mr. Knight observes, that there is an evident alteration in the character of leaves visible in the same variety, between those of two years and those of twenty years old; and that it is consequently highly probable that still further changes have occurred in the course of two centuries.

From these results respecting the importance of the leaves to the well-being of vegetables, the author ventures to suggest the probability, that the debilities of old age in animals may arise from a similar source, and may be traced to injury primarily sustained by the lungs.

It is not merely upon general analogy that such an opinion may be supported, but in particular instances of long life in men and in domesticated animals, it is observed that those individuals longest retain their health, and are most able to bear excessive labour without injury to their constitution, in whom the chest is manifestly most capacious.

On the Gizzards of Grazing Birds. *By* Everard Home, *Esq. F.R.S.* Read April 4, 1810. [*Phil. Trans.* 1810, *p.* 184.]

Since the organs of digestion in those quadrupeds which live wholly upon grass differ considerably in their construction from those

of other quadrupeds, and in greater or less degree according to the different qualities of their food, it was natural to expect some correspondent peculiarities in the gizzards of those birds which feed on grass, to fit them for digesting this kind of food.

With this view the author has examined the gizzards of the goose and swan, in comparison with that of the turkey, which feeds on a different kind of food.

For the purpose of rendering the fibres distinct, so as easily to be traced, the gizzards of each were boiled, after having been previously filled with plaster of Paris. In the turkey the two muscles, of which the gizzard consists, are of unequal strength, that on the left side being considerably stronger than that on the right. These muscles, by their alternate action, produce a constant friction on the contents; for though the direct pressure inwards is very great, the lateral motion occasions the force employed upon the substances contained, to be applied in an oblique direction, as Spallanzani and others have observed.

The internal cavity being of an oval form, like a pullet's egg, rounded on all sides, does not allow the opposite sides ever to come into contact; so that the food is triturated merely by the intermixture of bodies harder than itself.

In the goose and swan, on the contrary, the cavity is flattened, with its edges very thin. The surfaces applied to each other are, however, not plane surfaces; but a concave surface is applied to one that is convex; and in the left side the concavity is above; but the curvature changes, so that on the right side the concavity is below. In these gizzards the horny covering of their surface is much stronger than in the turkey, and rough; so that by a sliding motion of the parts opposed, the food is ground, although they do not admit the intervention of hard substances of a large size, and almost without requiring such assistance.

In the lower part of the œsophagus of these birds, the author observes an enlargement, which he considers peculiar to them, and thinks it answers the purpose of a reservoir, in which the grass is retained, macerated, and prepared, as in ruminating animals, for the subsequent process of rumination.

Observations on Atmospherical Refraction as it affects astronomical Observations; in a Letter from S. Groombridge, *Esq. to the Rev.* Nevil Maskelyne, *D.D. F.R.S. Astronomer Royal. Communicated by the Astronomer Royal.* Read March 28, 1810. [*Phil. Trans.* 1810, *p.* 190.]

Mr. Groombridge being in possession of a transit circle four feet in diameter, made by Troughton, undertook a series of observations upon circumpolar stars, for the purpose of determining the latitude of his observatory.

As his instrument had the advantage of being fixed upon stone piers, which are not liable to partial expansion, and as the size of the instrument itself seemed to him better adapted to determining the real quantity of atmospherical refraction than any which had been before employed for the same purpose, he extended the range of his observations as low down towards his north horizon as his situation would permit. For this purpose he selected fifty stars of different polar distances, and of these he made, upon the whole, upwards of 1000 observations.

The observed zenith distances being first corrected by the usual equations, so as to reduce them all to the same period, January 1, 1807, a correction is next made for refraction, according to Dr. Maskelyne's last precepts, in which the refraction at 45° is estimated at 56½″, with due allowance, as usual, for the states of the barometer and thermometer, as noted at the time of observation.

Since the co-latitude is equal to half the sum of the real zenith distances of any one star that has been observed, both above and beneath the pole, it is evident that the same result should be obtained from stars near the pole, as from those which are more distant, after all the requisite corrections have been rightly made. But since, by the author's observations, his co-latitude deduced from distant stars, which are subject to greater refraction, was found to be about 2½″ greater than from stars near the pole, he presumed that the allowance of 56½″ for mean refraction at 45° was too small. For if both the greater and less refraction be increased in the same ratio, the corrections thus made will be unequal, and their difference may be made to remove the inequality of the co-latitudes, as deduced from the mean of 56½″.

From the mean of 13 stars, which do not pass lower than 56° from the zenith, compared with the mean of 21 stars, between 60° and 78° zenith distance, Mr. Groombridge infers that the mean refraction is really as much as 58″ and a small fraction; and accordingly, in his table of observations, he gives corrections computed according to this supposition, whereby his column of co-latitudes is rendered uniform, without departing from the law of refraction at different altitudes laid down by Dr. Bradley.

The deductions thus made from observations on the fixed stars, are next compared with those obtained from the meridian altitudes of the sun at the solstices, which he thinks afford satisfactory proof of their correctness; as the latitude of his observatory, by the former method, was found to be 51° 28′ 2″·1, and by the latter 51° 28′ 2″·35.

The author proceeds to ascertain the difference of latitude between the Royal Observatory at Greenwich and his own, by comparison of his observations of the zenith distance of γ Draconis, with some of the same star communicated to him by Dr. Maskelyne; and by similar comparison of zenith distances of other stars observed at the Royal Observatory by Colonel Mudge with the zenith sector.

He next compares the refraction above deduced, with the results of other astronomers. Piazzi, having an instrument which turns in azimuth, has deduced the actual refractions at all distances from the zenith, by means of numerous observations on Procyon, α Lyræ, and Aldebaran, at various altitudes, from 38° to 89° zenith distance, in addition to several circumpolar stars. Piazzi's result is, that the mean refraction at 45° is 57″·3, which is less by eight tenths of a second than that of the author; but by the present French tables it is stated to be 58″·2, which, on the contrary, is rather greater. But beside the difference in the quantity of mean refraction at 45°, Piazzi observes that the law assigned by Bradley does not obtain; for though the actual refractions, so far as 80° from the zenith, are, in fact, greater than was supposed by Bradley, the refractions within the remaining 10° of the horizon are less than he supposed them to be.

In the series of observations given by the author, a similar want of conformity to Bradley's law is observable; and he observes, that the change of difference, from greater to less, takes place at 80° zenith distance, which is the same point of the heavens assigned by Piazzi.

Mathematicians, who have endeavoured to reconcile the known laws of refraction through different media, with the actual quantity deduced from observation, have proved that the refractions vary nearly as the tangents of zenith distance; but in order to reconcile this rule with the fact at low altitudes, they have found it necessary to introduce a correction of the zenith distance, and have invented a formula, consisting of a tangent of the zenith distance, diminished by some multiple of the refraction. The magnitude of this multiple has been estimated differently by different authors. By Simpson it is rated at 2·75; by Dr. Bradley 3; by Bouguer 3·23; by Cassini 3·226. Mr. Groombridge computes that this multiple should be as much as 3·3625.

In addition to the above endeavours to determine the mean refraction, and its variations at different altitudes, the author also considers the corrections which should be made for the states of the barometer and thermometer, and explains the means by which he deduced those that he has adopted, in order that any error therein may be more easily detected.

Extract of a Letter from the Rev. John Brinkley, *D.D. F.R.S.* Andrew's *Professor of Astronomy in the University of Dublin, to the Rev.* Nevil Maskelyne, *D.D. F.R.S. Astronomer Royal, on the annual Parallax of α Lyræ.* Read April 12, 1810. [*Phil. Trans.* 1810, *p.* 204.]

The principal object of Dr. Maskelyne in making this communication, is to inform the Society of a discovery, made by Dr. Brinkley, of the parallax of the annual orbit, which he has ascertained by observations on *α* Lyræ.

		Ann. Par.
The first seven observations were made near opposition, the next eight near conjunction........................	The comparison of these gave a result of	$2''{\cdot}18$
The next set were seven at opposition, and eight at conjunction	giving	$3{\cdot}06$
The last set eight at opposition, and eight at conjunction	gave	$2{\cdot}32$
		3) $7{\cdot}56$

So that by the result of 47 observations, the result is $2''{\cdot}52$; and Dr. Brinkley adds, that from the confidence which he has now acquired in his instrument, he has no doubt that the annual parallax exceeds $2''$.

This letter also contains some remarks upon refraction with the co-latitude of the Dublin Observatory, as deduced by means of different formulæ. From these it appears, that the agreement by Delambre's tables is nearer than by Dr. Bradley's own formula; but that Dr. Bradley's formula, by means of a slight correction, gives a table preferable to that of Delambre.

Dr. Bradley's is

$$56''{\cdot}9 \times \text{tang. (zen. dist.} - 3 \text{ Refr.)} \frac{\text{ht. barom.}}{29{\cdot}6} \times \frac{400}{350 + 6}$$

Dr. Brinkley's corrected formula

$$56{\cdot}9 \times \text{tang. (zen. dist.} - 3{\cdot}2 \text{ Refr.)} \times \frac{\text{ht. barom.}}{29{\cdot}6} + \frac{500}{450 + 6}$$

On the Mode of breeding of the Ovo-viviparous Shark, and on the Aëration of the fœtal Blood in different Classes of Animals. By Everard Home, *Esq. F.R.S.* Read June 7, 1810. [*Phil. Trans.* 1810, *p.* 205.]

With a view to understanding more fully the structure of the Squalus maximus, of which Mr. Home has lately published an account, he has examined with attention that of the Squalus acanthius, which appears to resemble it closely in its internal structure, and has the advantage of being very easily obtained upon the Sussex coast, where it is very common.

After describing minutely the external organs of generation in both male and female, the author traces the progress of the ovaria from the time that the yolks are no larger than peas, till they become as large as walnuts, when they pass into the oviduct. The number of yolks differs in different fish; and even in the same fish Mr. H. has seen five yolks in one ovarium and only two in the other. The oviducts then enlarge, and become exceedingly extended, and divided by contraction of its coats into three cavities, the last of which is ten inches in length, and is that in which the eggs are retained till the young fishes are formed, and capable of taking care of themselves.

The eggs, however, are not loose as in the oviducts of birds, but are grouped together to the number of three, four, or five, in membranous bags, containing a transparent jelly, in which the young fish swims about after it is formed, with the yolk attached to its belly by a long chord, consisting of blood-vessels. If the bag be torn and the fish taken out and put into water with its yolk attached to it, it swims about; but if the vessels of the chord are wounded, it dies immediately.

The author is of opinion that many of the shark tribe have this mode of hatching; but with respect to the large shark between the tropics, the fact has been already observed by Dr. Patrick Russell, who in one instance found twenty-one young ones in the right oviduct, and twenty in the left.

The gelatinous liquor surrounding the ova being found to differ in its properties from their animal jellies, excepting that with which the ova of frogs are surrounded, Mr. H. procured some frogs that he might watch the formation of their jelly, and examine its properties. No change was observed to take place in their ovaria through the winter, but on the 10th of February when a portion of oviduct was immersed in water at 80°, it swelled to double its size, and even larger when the water was warmer. On the 25th of February a portion of oviduct, only two or three inches long, being put into water at 120°, swelled to such a mass of transparent jelly as filled a half pint tumbler. This substance resembled what is occasionally found on the ground, and on the branches of trees, and is called star-shot jelly; which by Pennant has been supposed to be brought into that state in the stomachs of herons and of other birds that feed on frogs, and then rejected by vomiting.

Mr. Brande was consequently requested to make a comparative examination of the jelly from the shark, of the jelly obtained from the oviducts of frogs, and of star-shot jelly, procured from Lincolnshire; and he found them to agree perfectly in their properties.

When dried they become brittle, but when put into water, they expand again to their original bulk, even although the heat of boiling water has been applied for drying them. Water does not appear to dissolve any portion of them even by boiling; they dissolve, however, by acids or by alkalies. As none of the solutions are precipitated by tannin, the substance distinctly differs from gelatin; and as it is not coagulated by heat, by acids, by alcohol, or by electricity, it differs equally from albumen, and must be considered as of a peculiar nature not yet described.

The subsequent part of Mr. Home's communication relates to the provisions for supplying the fœtus of different animals with air.

The ova of many fish are laid as near the sources of rivers as they can be, for the sake of the greater proportion of air contained in the water. Others are attached to plants which assist in supplying them with oxygen. The ova of sharks and of skates, which have their coats too strong to be penetrated by sea-water, have apertures at each end for its admission and escape.

In the ovo-viviparous sharks, which are the subjects of the present communication, the ova being contained till hatched in the body of the fish, have no hard covering, and are surrounded by sea-water, which has admission into the oviduct, by an aperture for that purpose.

In the kanguroo, and others of the opossum tribe in New South Wales, there is a communication between the uterus and vagina, which answers the same purpose of aërating the fœtal blood, which is necessary in these animals; because the young is not, as in others, connected with the uterus by a navel-string, but is detached as a sort of soft egg, and consequently cannot receive the influence of the arterial blood of the mother through the coats of any contiguous vessels.

On Cystic Oxide, a new species of Urinary Calculus. By William Hyde Wollaston, *M.D. Sec. R.S.* Read July 5, 1810. [*Phil. Trans.* 1810, *p.* 223.]

The calculus, which is the subject of the present essay, is the only new species which the author has had an opportunity of observing, in addition to five which he described to the Society in the year 1797.

It appears to be comparatively very rare, as he has hitherto seen only two specimens of it; one in the possession of Dr. Reeve of Norwich, and the other in a collection of calculi belonging to Guy's Hospital. They are in appearance more like the triple phosphate of magnesia than any other calculus, but are more compact and semi-transparent, with a slight tinge of yellow.

By destructive distillation they yield fœtid carbonate of ammonia, with a heavy animal oil, and the residuum is a black spongy coal.

They are not dissolved by water, by alcohol, by acetic acid, by tartaric acid, but are dissolved by most other acids, by muriatic, nitric, sulphuric, phosphoric or oxalic acids.

They are dissolved also by most alkaline menstrua, as by solutions of pure potash or soda, pure ammonia, or by lime-water, and even by the fully saturated carbonates of potash or of soda, but not by carbonate of ammonia.

It is remarked also, that the solution formed with nitric acid does not yield oxalic acid, as the uric acid does when similarly treated, and does not turn red in drying, but becomes brown, and ultimately black, when much heated.

Since this substance yields carbonate of ammonia by distillation, it evidently contains oxygen, but it does not appear acid, as it does not redden litmus, but has rather the properties of an oxide, inasmuch as it unites readily with either acid or alkaline substances; and the author is induced to give it the name of cystic oxide, because the only calculi hitherto observed have been taken from the bladder.

The author takes this opportunity of correcting an inaccuracy or two in his former communication on this subject; and he also adds

some observations respecting the different quantities of uric acid voided by birds living upon different kinds of food, not being produced by those that live entirely upon fish.

Researches on the oxymuriatic Acid, its Nature and Combinations; and on the Elements of the muriatic Acid. With some Experiments on Sulphur and Phosphorus, made in the Laboratory of the Royal Institution *. *By* H. Davy, *Esq. Sec. R.S. Prof. Chem. R.I. F.R.S.E.* Read July 12, 1810. [*Phil. Trans.* 1810, *p.* 231.]

The tendency of the author in the present investigation, is to return to the opinion respecting the relation of muriatic acid and oxymuriatic acids to each other, which was originally entertained by Scheele.

According to that most illustrious chemist, the oxymuriatic was the more simple body, and by union with phlogiston became muriatic acid. But from many experiments made soon after by Berthollet, it was inferred that the latter was simple, and by union with oxygen became converted into oxymuriatic acid. In Mr. Davy's former attempts to obtain the base of muriatic acid by potassium, he has not been able to separate anything from it but hydrogen. In Dr. Henry's endeavours by electricity to decompose the muriatic acid, hydrogen and oxymuriatic acid were evolved; and conversely, Mr. Davy has in no instance been able to separate oxygen from oxymuriatic acid, or even to separate muriatic acid from dry muriates, without the assistance of hydrogen or water. He has hence been led to doubt the existence of oxygen in the substance called oxymuriatic acid, and has applied the most powerful means of abstracting oxygen from it without success; and indeed Messrs. Gay-Lussac and Thenard, in their elaborate and interesting experiments, published in the Memoires d'Arcueil, although they maintain that muriatic acid gas consists of muriatic acid and water, are not able to separate water from it, but only hydrogen; and themselves acknowledge that oxymuriatic acid, which they suppose to consist of muriatic acid and oxygen, cannot be decomposed by any known means.

The most extraordinary fact noticed by Mr. Davy is, that when charcoal is ignited to whiteness by the voltaic battery in oxymuriatic acid gas, no change whatever is produced, provided that the charcoal has been previously freed from moisture or from hydrogen by intense heat.

The vivid combustion of many bodies in this gas has favoured the presumption that it contained oxygen very loosely combined and ready to exert its utmost power of affinity: but it is mere presumption; since heat and light result also from the intense agency of any other combination, without the presence of oxygen.

The resemblance of oxymuriatic acid combined with metals to other neutral salts, may be considered a strong argument in favour

* Communicated to the Royal Society at the request of the Managers of the Royal Institution.

of the presence of oxygen in that acid: but Mr. Davy observes, that an opposite doctrine may be equally maintained; since the metals may consist of bases united with hydrogen, which, by combining with oxymuriatic acid, converts it into muriatic acid. A corresponding doubt occurs also respecting the nature of hyperoxygenized muriatic acid. Does the oxymuriatic acid combine with oxygen as well as with hydrogen? and does it, with the former, produce hyperoxygenized muriatic acid? or is the hyperoxygenized acid the base of this class? and does this unite with different proportions of hydrogen? In order to answer these questions, Mr. Davy has endeavoured to obtain the neutralizing acid in hyperoxygenized muriate of potash, by distillation with dry boracic acid; but in this case, oxygen is the chief gaseous product, and there remains common muriate of potash, not decomposable by any dry process. Other attempts were also made to solve the same problem, by attending to the phenomena which occur in the decomposition of various compounds by the agency of voltaic electricity. The fact most favourable to the existence of hydrogen in oxymuriatic acid takes place during the electrization of oxymuriate of tin. Hydrogen, which in this case is extricated, must be produced either by the metal or by the oxymuriatic acid.

No substance, says Mr. Davy, has less claims to be considered an acid than oxymuriatic acid; and he considers it a body *sui generis*, that has a tendency to combine with pure inflammable matters, forming what are called the dry muriates. Of this class are the common metallic muriates, the muriates of potash, of soda, lime, strontites, and barytes. But there are other bases which in their state of oxide unite with muriatic acid gas, and retain the water which is formed in their composition. Such are, the muriates of ammonia, of magnesia, of zircon, alumina, and yttria.

Although the modern chemical nomenclature accords extremely ill with these views of the composition of the several forms of muriatic acid, and of muriatic salts, the author does not venture to propose any alterations in their names until their real nature shall be more fully known.

Among the various compounds of oxymuriatic acid with combustible substances, there is one which the author has examined with peculiar care, and with very singular results. He has on a former occasion examined the action of oxymuriatic acid upon phosphorus, and has described two compounds, the one fluid, the other solid; the first of which, according to the generally-received theory, should consist of muriatic acid and phosphorous acid; and the second of muriatic acid and phosphoric acid. If such were really the case, he imagined it would not be difficult to obtain the phosphoric acid as proof of the presence of oxygen; and he accordingly saturated the compound with pure ammoniacal gas, expecting to obtain muriate of ammonia, and phosphate of ammonia, which, by being heated, would leave the phosphoric acid in a pure state.

The triple compound formed was, on the contrary, a dry powder, not fusible by a red heat, nor yielding any gaseous matter when

heated, and not decomposable by mere heat. It had no taste, no smell; it did not seem to be soluble in boiling water, nor even in muriatic, nitric, or sulphuric acid; neither did strong lixivium of caustic potash appear to produce the least effect; and the only processes by which it appeared to be affected were combustion, or the action of fused potash. In the latter case it emitted a smell of ammonia; it appeared to dissolve in the potash, which then gave indication of the acids that had entered into the composition of this singularly intractable substance.

Amongst the known combustibles, it is observed that charcoal is the only one which does not combine directly with oxymuriatic acid gas; but Mr. Davy is of opinion that it does in some cases unite by the medium of hydrogen, as in the state of olefiant gas, and in the formation of muriatic ether. The author expresses a hope that new and more correct views of the composition of muriatic salts will facilitate their decomposition, and explain, in a satisfactory manner, various economical processes, long since practised, for obtaining the acid from common salt by means of aluminous or siliceous substances, the success of which has in general depended on the accidental presence of moisture. In this case the alkali forms a glass, and is rendered useless; but by the substitution of iron filings, and passing steam over the mixture when heated, Mr. Davy has succeeded in separating a portion of soda from common salt.

Observations upon Luminous Animals. By J. Macartney, *Esq. Communicated by* Everard Home, *Esq. F.R.S.* Read May 17, 1810. [*Phil. Trans.* 1810, *p.* 258.]

Although the property of emitting light, which is possessed by some animals, has appeared interesting to naturalists of all ages, and although many detached memoirs have been written upon the subject, the author observes that the history of these animals is still extremely imperfect; and he endeavours to supply the defect by enumerating all the different animals which, to his knowledge, possess that property, and to ascertain, by dissection or otherwise, the parts of their bodies from whence the light issues, and, by experiment, to discover the circumstances necessary or accessory to the emission of light.

The genera in which individuals certainly luminous are to be found are as many as twelve in number. There is one species of Pholas amongst the mollusca. Among insects are seven genera; Elater, Lampyris, Fulgora, Pausus, Scolopendra, Cancer, and Monoculus. Among the worms is one, Nereis. And in the class of zoophytes, the three genera of Medusa, Pyrosoma, and Pennatula.

The same property has also been ascribed to various kinds of fish; but, in the author's estimation, they have probably acquired that reputation by occasionally evolving light after death.

It has also been said, that the *Lumbricus terrestris*, or common earth-worm, has been found to be luminous for several days together;

but it does not seem probable that such a property (if it existed) could be overlooked in an animal so extremely common.

The luminous property of the *Pholas dactylus* was observed by Pliny to be communicated to the hand that touched it; and this has been confirmed by Reaumur.

Of the genus Elater there are three species luminous, one of which, the *noctilucus*, is so brilliant, that it was employed by the South American Indians for the purposes of illumination.

In the genus Lampyris there are many species, at the head of which is the *Lampyris noctiluca*, or common glow-worm, which may serve as a specimen of the manner in which the light is produced by the rest. In this species the light lasts only during the breeding season; and as it is confined to the female insect, it would appear to answer the purpose of guiding or inviting the male. But it has been observed by Montbeillard, that the eggs are also occasionally luminous; and Mr. Macartney confirms this observation, as he once saw a quantity of them shine unremittingly for several days together.

Of the genus Fulgora, only three have been particularly noticed for the light they yield,—the *lanternaria*, *candelaria*, and *pyrorhynchus*; although it is probable, from their structure, that many others of the genus possess this property.

The Pausus is remarkable only for the situation of its light, which is carried at the extremity of its antennæ.

The *Scolopendra electrica* is not uncommon in this country; and yet the light has not often been observed. According to the author's observation, the light is communicated, and remains visible for some time on a hand that has touched it; and, in one or two instances, insects which had been long confined from the light did not appear luminous, but acquired this faculty after being for some time exposed to daylight.

The *Cancer fulgens* was discovered by Sir Joseph Banks, who observed that its whole body was illuminated, and produced very vivid flashes of light.

Of the Monoculi, the author reckons three species that are luminous; one discovered by Godeheu de Riville, and two by Capt. Horsburgh.

With respect to the *Nereis noctiluca*, to which the light of the sea has been ascribed by Vianelli, Griselini, Spallanzani, and others, in various parts of the Mediterranean, and by Adleo in the African and Indian oceans, the author is of opinion that it never appears on the coasts of this country, and that the light of the sea, which we most frequently witness, is caused by Medusæ.

Of these, the largest is the *Medusa pellucens*, discovered by Sir Joseph Banks, along with the *Cancer fulgens*, in his voyage with Capt. Cook between Madeira and Rio de Janeiro. This is six inches in diameter. The *noctiluca* described by Taskal is three inches in diameter.

Another Medusa was discovered by Spallanzani in the Straits of Messina: it is said to be as bright as a torch, and visible at the depth

of thirty-five feet below the surface of the sea. As this light occasionally disappears entirely, and reappears after a considerable interval, Spallanzani supposed the animal to be luminous only when in motion, and that the cessation depends upon its being at perfect rest.

The *Pyrosoma Atlanticum*, discovered by Peron, seems confined to certain latitudes, and its light, like that of the former, is supposed to be visible only during motion.

Mr. Macartney has himself discovered one Beroe not before observed, and two Medusæ, unless one of the latter be, in fact, the *Medusa hemisphærica* of Gronovius and of Muller, who did not perceive it to be luminous. The second Medusa, from its extreme minuteness and brilliancy, he calls *Medusa scintillans*: on account of its smallness it cannot be separated from the water but by straining through a cloth. When a small number of them are put into clear water, it is difficult to distinguish them while separate, on account of their minuteness and transparency; but as they gradually collect at the surface of the water, they then appear together of a dusky straw colour.

It is to this species of Medusa that the author is inclined to ascribe many phenomena of illumination of large portions of the sea which have been described by navigators.

In some instances the sea has been compared to a plain of snow. Capt. Horsburgh saw it of an uniform white colour, like milk, on the Malabar coast, and says it has frequently that appearance in the Banda Sea. The same was observed by Mr. Langstaffe in a voyage from New Holland to China; and it was ascertained by him to arise from numerous minute bodies of the size of small pins' heads, which, when lifted out of the water by adhering to the hand, were found connected together as a chain.

At Margate the author has seen these in great abundance; and in certain parts of Milford Haven they are generally so numerous, that on one occasion he separated, by straining, a pint of Medusæ from a gallon of the water.

Mr. Macartney next examines the particular structure of those insects which possess distinct organization for the production of light; as, the various species of Lampyris, Elater, Fulgora, and Pausus.

The light of the genus Lampyris resides generally in the last rings of the abdomen. In the common glow-worm these rings are very transparent, and there is diffused over their internal surface a yellowish substance that has been compared to paste; but the author observes that the matter is organized. He also remarks, that in the last ring of the abdomen there are two small sacs that emit a light brighter than the rest, and apparently less under the control of the will, as it is more permanent.

In the *Elater noctilucus* and *Elater ignitus*, there is a similar soft yellowish substance underneath the corselet. But in those of the genus Fulgora, which the author has had opportunities of examining, this peculiar matter was not distinctly observable.

In none of these animals could Mr. Macartney discern that the luminous organs were supplied either with nerves, or with air tubes, better than other parts of their bodies.

With the exception of these insects, in which the light is confined to particular parts, the exhibition of light appears to the author to depend on the presence of a fluid matter diffused throughout the whole body of the animal.

By squeezing the fluid of two large Medusæ into a glass of well-water, it was rendered luminous for nearly an hour and a half. Agitation also occasioned a fresh appearance of light after that period, and even after agitation ceased to produce any effect, an increase of temperature rendered it again luminous for a short time.

Since the phenomena of animal light have been attempted to be explained in various ways by different authors, and since their experiments are in many respects at variance with each other, the author adds a series of experiments of his own, from which he draws his own conclusions.

A glow-worm lived, and emitted light, nearly two hours in a glass of water, though thus confined from oxygen.

The luminous substance, after being extracted from glow-worms, gave no light.

The luminous sacs, on the contrary, after having been cut out from the tail of the glow-worm, continued to emit light several hours in the atmosphere; and when put into water the power was prolonged to forty-eight hours.

By application of heat they were not rendered more luminous, and had no tendency to active combustion, like phosphorus.

The luminous part of the glow-worm appeared to raise a delicate thermometer, somewhat more than other parts of its body; but of this fact the author is not confident; but when the luminous sacs had been separated from the body of the animal, these had no effect on the thermometer, though they continued to emit light.

By heating some water containing Medusæ, the brilliancy of their light was very much increased; but they were killed in less than a minute.

Some of the same Medusæ, being put into spirits of wine, emitted immediately a strong light, which continued till they died.

The *Medusa scintillans*, or *hemisphærica*, being put under the receiver of an air-pump, in a vessel of water, continued to emit light, notwithstanding complete exhaustion of the air.

A Medusa, upon being electrified by sparks from an electric machine, was not excited to give out light. But when shocks were transmitted through a collection of *Medusæ hemisphæricæ*, they were excited, and shone with great brilliancy.

From these experiments, says the author, it appears that the luminous substance is by no means of the nature of phosphorus, as it often shows the strongest light when excluded from oxygen gas; and so far from undergoing any process of combustion, it is incapable of being inflamed; that the increase of heat during the shining of glow-

worms is merely an accompaniment, not an effect of the phenomenon; and that heat and electricity act merely like other stimuli upon the vital powers of the animal.

Observations and Experiments on Pus. By George Pearson, *M.D. F.R.S.* Read July 5, 1810. [*Phil. Trans.* 1810, *p.* 294.]

The author prefaces the account of his experiments and observations on the nature and properties of purulent fluids, by an etymological disquisition concerning the origin of the word *Pus*, and the various senses which philologists may discover for the word πυος, besides the distinct signification given to it by Hippocrates, of a thick, white, inodorous, uniformly smooth fluid, which is contained in an abscess. From the etymology, Dr. Pearson next proceeds to the history of the several opinions that have been entertained respecting the formation of purulent matters, and of the characters by which different persons have endeavoured to distinguish real pus, from such purulent fluids as ought rather to be considered as modifications of mucus. Since nothing appears to have been added since the date of Mr. Home's dissertation on pus, which was written in the year 1798, Dr. Pearson's history concludes with an outline of Mr. Home's account of the nature of pus. According to him, pus is composed of globules swimming in a transparent aqueous fluid. The globules, on which its opacity depends, are formed subsequently to the secretion of the transparent fluid. They are not soluble in cold water, like those of blood, but are decomposed by boiling water; and the fluid in which they swim is not coagulable by heat, as serum, but is coagulable by sal-ammoniac, which does not coagulate serum.

Dr. Pearson's examination of pus is divided into six sections, of which the first treats of the simple and obvious properties; and he distinguishes four different kinds of pus.

1. The cream-like and equally consistent.
2. The curdy of unequal consistence.
3. The serous, or thin kind.
4. The thick, viscid, or slimy.

Of course, as he examines, under the name of pus, fluids so different from each other, he obtains results which differ accordingly in the qualities and quantities of their ingredients.

In the second section the agency of caloric is observed.

According to the author, all kinds of pus are coagulated between 160° and 165° of Fahrenheit. By continued heat the water is evaporated, and there remains a dry brittle mass, amounting to about one seventh or one eighth of the original weight. By exposure to greater heat in a crucible of platina, the greatest part of this residuum was consumed, and there remained only the salts of the serum fused together, and amounting to [illegible]th, [illegible]dth, or [illegible]dth, of the original quantity of pus employed.

These, says the author, consisted chiefly of muriate of soda, phosphate of lime, potash, with strong indication of carbonate of lime,

and a sulphate, beside traces of phosphate of magnesia, oxide of iron, and vitrifiable matter (probably silica).

The different kinds of pus are next mixed with large quantities of water, and the matter which subsides examined separately from the water. In the same manner they are next mixed with alcohol, and afterwards with acetic acid, but without any remarkable results. Dr. Pearson also made various attempts to discover a criterion by which to distinguish pus from mucus; but after trying the agency of sulphuric, nitric, and muriatic acids, he says he could by these discover no constant characteristic property of these substances by such experiments.

By alkalies also, he was not more successful; nor did the subsequent addition of acids to the solution afford criteria which could be depended upon, as has been supposed by other experimentalists. After trial of several different neutral salts, he observed the same effect from sal-ammoniac which had been noticed by Mr. Hunter, and was considered by him as coagulation: but Dr. Pearson gives it the name of inspissation, and observes, that this effect is not produced in expectorated matter by the same salt; so that this is undoubtedly a criterion, as it was supposed to be by Mr. Hunter.

From the whole of his experiments, Dr. Pearson infers that pus essentially consists of three distinct substances: first, an animal oxide in the form of fine curdy particles, not soluble in water, not coagulable into one mass by hot water; secondly, a limpid fluid, like serum of blood, and like it coagulable by heat or by alcohol; and thirdly, innumerable spherical particles, visible only by the microscope, not soluble in hot or cold water, and specifically heavier than water.

He observes also, that other extraneous matters are sometimes mixed with pus: that whenever pus is fetid, it is in the state of putrefactive fermentation; that the several ingredients in pus are products of secretion; that the varieties of purulent matter depend on the proportion of its essential ingredients; that the saline ingredients before named are dissolved in the serous fluid, and that the quantity of these is less than in an equal quantity of expectorated matter, but more than in an equal quantity of serum of blood.

That besides the consistence of pus depending upon the proportion of its essential ingredients, some difference may also arise from the mode or state of coagulation of the matter which gives the opacity.

According to the above inferences, the author conceives that a distinct and definite notion of the substance to be considered as pus is exhibited, and that it will now be readily ascertained what is and what is not pus, by a few easy experiments.

But since it is frequently disguised by the admixture of other matters, and a degree of ambiguity arises, especially in pulmonic diseases, he concludes by endeavouring to elucidate the subject, by remarks on puriform matters expectorated in different cases of pulmonary disorders.

The Bakerian *Lecture. On some of the Combinations of Oxymuriatic Gas and Oxygen, and on the chemical Relations of these Principles, to inflammable Bodies. By* Humphry Davy, *Esq. LL.D. Sec. R.S. F.R.S.E. M.R.I.A. and M.R.I.* Read November 15, 1810. [*Phil. Trans.* 1811, *p.* 1.]

Mr. Davy, having in his last communication to the Society expressed his belief that the substance called oxymuriatic acid gas has not yet been decompounded, but is simple, as far as our present knowledge extends, and having been confirmed in that opinion by subsequent experiments, endeavours, on the present occasion, to select such experiments as tend to illustrate more fully the nature, properties, and combinations of this substance with inflammable bodies, and compares its properties with those of oxygen, to which he considers it as bearing the closest analogy.

When potassium is exposed to oxymuriatic gas, the intensity of their mutual attraction occasions spontaneous inflammation. Ten grains of potassium absorb about eleven inches of the gas; and they form a neutral compound, which is the same as muriate of potash which has been ignited. When this metal or sodium are burned in oxygen gas, the combustion is much less vivid, since their attractions for oxygen are feebler than for oxymuriatic gas; and the alkalies, potash, and soda, are formed in a state of extreme dryness; but under certain circumstances they are liable to combine with an excess of oxygen, and to become peroxides, as observed by Messrs. Gay-Lussac and Thenard.

The oxides, when newly formed, being perfectly dry, require a strong red heat to fuse them. When small quantities of water are added to them, they heat violently, and are converted into hydrates that are easily fused, and are in a certain degree volatile.

By ignition they do not lose the whole of the water, but retain a portion, as has been observed by M. Berthollet and M. D'Arcet. Mr. Davy's method of ascertaining the quantity of water retained, was by means of the boracic acid, previously dried by heating to whiteness for nearly an hour; and he found about 16 per cent. in potash, and about 23 in soda. But when potassium, or potash recently prepared from potassium, was employed, and combined with dry boracic acid, no moisture whatever was extracted. It is evident, therefore, that common potash and soda are hydrates, and that the compounds formed by the combustion of the alkaline metals are pure metallic oxides, free from water.

If one grain of potassium be burned in oxygen gas it absorbs half a cubical inch, and if the oxide so formed be subsequently exposed to oxymuriatic gas, then one and one eighth cubic inch of this gas are absorbed, and the half cubic inch of oxygen is extricated. When dry potash, or peroxide of potassium, are heated in oxymuriatic gas, no moisture is extricated, excepting when the gas itself contains aqueous vapour. But when muriatic acid gas was introduced

to potash formed by the combustion of potassium, then water was instantly formed, and oxymuriate of potassium.

The phenomena when sodium or soda are employed, are precisely analogous to the former; but the quantity of oxygen absorbed by sodium, and extricated from it by oxymuriatic gas, is very nearly twice as much as with potassium.

When two parts of potassium are heated with common salt that has been previously dried, the salt is decompounded; and one part of sodium is obtained in a very pure state by an extremely easy process.

From the experiments on sodium contained in the last Bakerian lecture, Mr. Davy deduces the elementary number 22, as representing the proportion in which it unites with different bodies.

He observes also, that the proportions ascertained on the present occasion to exist in the hydrates of potash and of soda, accord with the supposition that they each contain one part of water, combined with one of the respective alkali.

The proportions also of potash or soda in different neutral combinations by these estimates (says Mr. Davy), will be found to agree very nearly with those derived from the most accurate analyses; and as one instance, he refers to Dr. Marcet's analysis of muriate of soda.

Since the muriates of barytes, lime, and strontia, when thoroughly dried by exposure to a white heat, are not decomposable by boracic acid, or by any simple attractions, Mr. Davy was led to suppose that they consisted of their peculiar metallic bases, combined with oxymuriatic gas; and he is confirmed in this opinion by the result of other experiments; for when these earths are heated to redness in oxymuriatic gas, the same dry muriates are formed, and oxygen is expelled. The proportion which this oxygen bears to each earth, Mr. Davy has not yet ascertained; but he found it to be in the constant ratio of one to two in volume of the oxymuriatic gas employed.

When dry quick-lime was heated in muriatic gas, water was immediately formed; and it can hardly be doubted, says Mr. Davy, that this arose from the union of hydrogen from the acid with oxygen from the lime.

The author next endeavoured to obtain the metals of barytes, strontia, and lime, from their muriates by means of potassium; and though he did not succeed in separating them, he is of opinion that either the bases of the earths were wholly or partially deprived of oxymuriatic gas by this process, or that the potassium had entered into triple union with their muriates.

When small portions of the common metals were heated in oxymuriatic gas, they each inflamed, with the exception of gold, silver, and lead.

The product from arsenic was butter of arsenic highly volatile; that from antimony was butter of antimony easily fused, but crystallized when cold. Those from tellurium, zinc, and bismuth, were very similar to the preceding.

The product from mercury was corrosive sublimate.

The compound formed with iron was of a bright brown, iridescent, like the Elba iron ore, volatile at a very moderate heat, and forming brilliant crystals on the sides of the vessel.

Tin afforded Libavius's liquor, having its usual properties.

When instead of the metals themselves their oxides were exposed to the action of oxymuriatic gas, oxygen was given off, in most cases at a heat below redness; and the quantity was the same as had been previously absorbed by the metals.

From the whole of the present series of experiments, Mr. Davy derives confirmation of his former opinion respecting the simple nature of oxymuriatic gas, the leading property of which is its tendency to unite with inflammable bases, forming binary compounds. Since its affinity is in most cases greater than that of oxygen, it either produces the expulsion of the oxygen, or causes it to enter into new combinations. He considers the oxygen that is expelled to arise from the oxide, because it is in proportion to the quantity which the oxide contained, and bears no relation to the quantity of acid.

If the oxymuriatic gas consisted of any acid matter combined with oxygen, its acidity, says Mr. Davy, should appear when it is united to phosphorus. But when two parts of this gas are combined with one of phosphorus, the compound has no effect on litmus paper, and does not act on dry lime or dry magnesia. But by union with hydrogen it forms an acid; in the same manner oxygen communicates acid properties to sulphur and phosphorus.

Although its affinities are in general superior to those of oxygen, Mr. Davy notices some exceptions. For instance, the boracic and phosphoric acids are not decomposed by it; and the same appears to be the case with the peroxides of iron and arsenic.

Of all the different bodies supposed to be elementary, nitrogen is that which has the weakest tendencies to combination in general; and it does not hitherto appear to have any affinity for oxymuriatic gas.

With respect to the simple or compound nature of nitrogen, Mr. Davy has not yet arrived at any satisfactory conclusion, although the general result of his attempts to decompose it has shown an apparent evolution of hydrogen, and other effects which could only be ascribed to the presence of oxygen.

But, on the contrary, the numerical expression for nitrogen, which corresponds to 13·4, does not accord with any simple proportion of oxygen and hydrogen; and this, together with other circumstances, occasion Mr. Davy to resist the inference of its being decompounded.

The author concludes with reflections on the nomenclature of oxymuriatic gas, and its compounds; since it appears to him that an alteration is necessary to assist the progress of discussion, and to diffuse just ideas on the subject.

It is to be regretted that the great discoverer of this substance did not originally affix to it a simple name to which we might now recur; but his term dephlogisticated can hardly be adopted in the present state of science.

Mr. Davy, preferring some name founded upon one of its obvious and characteristic properties, proposes *chloric gas*, which does not imply any error, and would not require to be changed, even if it should hereafter be discovered to be a compound.

For expressing the compounds of this substance with other bodies, he is not disposed to employ the same term, but proposes adding to each base the terminal syllable *ine*, which is to imply the presence of the chloric base. Thus horn silver is to be called argent*ine*; butter of antimony, antimon*ine*, &c. He conceives also, that by means of vowels prefixed to the name, the proportion in which this body is combined with others may be conveniently expressed.

The Croonian *Lecture. On some Physiological Researches, respecting the Influence of the Brain on the Action of the Heart, and on the Generation of animal Heat. By Mr.* B. C. Brodie, *F.R.S.* Read December 20, 1810. [*Phil. Trans.* 1811, *p.* 36.]

It has been observed by Mr. Cruickshank, and the same observations have been made by M. Bichat (in his *Récherches Physiologiques sur la Vie et la Mort*), that the brain is not directly necessary to the action of the heart; and that when the functions of the brain are destroyed, the circulation of the blood ceases only in consequence of the suspension of the respiration.

The former of these observations Mr. Brodie had found to be correct; for if the spinal marrow were divided, though the respiration was thereby immediately stopped, still the heart continued to contract, and to propel forward, for a short time, dark-coloured blood; and even when the head was entirely removed, if the blood-vessels were secured by ligature, the circulation seemed unaffected by the entire separation. It appeared, therefore, in conformity to the second observation, to cease solely in consequence of the suspension of respiration; but Mr. Brodie conceived that this point might admit of direct proof by experiment; for in that case the heart should continue to act for a greater length of time, if the process of respiration were carried on artificially.

The present lecture comprises the details of his experiments on this subject.

The first experiment was made upon a rabbit, the head of which was removed after the blood-vessels had been tied up; and the lungs were then inflated artificially once in five seconds, during twenty-five minutes. The circulation of the blood was found to continue the whole of that time; but it was observed that no secretion of urine took place.

The second experiment was made upon a middle-sized dog, for the purpose of ascertaining also, whether the animal heat was kept up to its natural standard. At the end of two hours the pulse continued as high as seventy, but in the next half hour it was found to have declined rapidly, and the artificial respiration was discontinued. At the end of one hour a thermometer in the rectum had fallen 6°;

at the end of two hours it had fallen 14° in the thorax; and at the end of the experiment 20° in the thorax.

At the beginning of this experiment the ureters had been tied; and at the end of the experiment it was found that no urine was collected above the ligature.

The third experiment was made upon a rabbit; and the artificial respiration was continued one hour and forty minutes. A thermometer in the cavity of the abdomen fell in the first hour from 100° to 89°; and in the next forty minutes to 85°; but in the cavity of the thorax it was as low as 82°.

Since the blood in these experiments was observed to retain its florid red colour in the arteries, it might be expected, according to the common theory of animal heat, to retain also its proper temperature; but Mr. Brodie observes, that this must also depend on the fulness and frequency of the pulse, together with the fulness and frequency of inspiration. It therefore became necessary to attend particularly to these circumstances.

In the fourth experiment, which was also made upon a rabbit, the natural inspirations were imitated as nearly as possible; and at the end of forty minutes the pulse was found to continue as high as 140 in a minute, but the heat had declined from 99° to 92½°. At the end of one hour and twenty-five minutes, a thermometer in the pericardium was 85°, in the abdomen 87°.

In the fifth experiment the pulse continued at 140 for upwards of an hour, but the heat in the rectum declined from 101½° to 92°.

In a subsequent experiment two rabbits were chosen, as nearly alike in size and every particular as possible. Both were killed; but one was suffered to cool gradually without interruption; and in the other the circulation was continued by means of artificial respiration.

At the beginning of the experiment the temperature of each was 99°.

At the end of half an hour that of the former was	99°;	the latter	97°
Three quarters of an hour	98	———	95½
One hour	96½	———	94
One hour and a half......................	94	———	91

It appeared, therefore, that respiration tended rather to cool than to support the animal heat. But since it was possible that a small portion of heat might be generated, but counteracted by the contact of cold air, a subsequent experiment was made, in which the large vessels of the heart were tied, so as to prevent any circulation of the blood; and in this case the heat remained comparatively greater than in the former, as the cooling effect of the air was not diffused to distant parts of the body.

From the whole the author concludes,

1st. That the influence of the brain is not directly necessary to the action of the heart.

2nd. That the interruption of the circulation is owing to the stoppage of respiration.

3rd. That when the influence of the brain is cut off, the secretion of urine ceases, and the production of animal heat is discontinued, even though the blood is preserved of its florid red colour.

4th. That, on the contrary, the coldness of the air applied is communicated to the blood, and thereby diffused to distant parts of the body.

On the Expansion of any Functions of Multinomials. By Thomas Knight, *Esq. Communicated by* Humphry Davy, *Esq. LL.D. Sec. R.S.* Read June 7, 1810. [*Phil. Trans.* 1811, *p.* 49.]

As M. Arbogast is the only author who has cultivated this part of analysis with any great success, it appeared desirable to the author to take a different view of the same subject, in order to confirm Arbogast's results by a different mode of obtaining them.

His own method has also the further advantage of arriving at several new and remarkable theorems (particularly with respect to inverse derivation), which probably could not be found by the method of M. Arbogast.

As far as concerns the functions of a single multinomial, the rules are the same as those of Arbogast; but in the more difficult cases of double and triple multinomials and functions of any number of them, the methods of the author are professed to be new and expeditious; and they are demonstrated with a great degree of facility and simplicity, from the analogy which reigns throughout his manner of treating the subject, and which enables the reader more readily to retain the rules in his memory.

On a Case of nervous Affection cured by Pressure of the Carotids; with some physiological Remarks. By C. H. Parry, *M.D. F.R.S.* Read December 20, 1810. [*Phil. Trans.* 1811, *p.* 89.]

In the year 1788 Dr. Parry published, in the Memoirs of the Medical Society of London, an account of many symptoms, such as headache, vertigo, mania, dyspnœa, convulsions, and others usually denominated nervous, that had been removed by pressure on the carotid arteries, which the author conceives to have operated by diminishing a too violent impulse of blood into the vessels of the brain, and thereby obviating excessive irritation.

From various cases which have occurred to Dr. Parry since that period, he selects one which appears to him to afford a singular illustration of the principle. It is that of a lady, who, after having been exposed to severe cold for some time, was seized with numbness of the left side, succeeded by tingling of the left hand, and deafness of the left ear, succeeded by excessive sensibility to sound. These were followed by a feeling of contraction or stiffness of various muscles of that side, and subsequently flutterings and twitchings of the flexor muscles of the fore-arm and of the deltoid; not, however, so as to

move her arm or hand. The rate of these vibrations was usually about 80 in a minute; but were much increased from any slight cause of general irritation.

Upon examination of the carotids, they seemed to be somewhat dilated, for about half an inch in length; but in other parts they were not larger than natural. The involuntary motions, which in this lady were confined to the left side, were not in any degree affected by pressure of the carotids on that side; but when the right carotid was strongly compressed, all the vibrations were uniformly stopped; which, says the author, hardly could occur but from removal of undue pressure of the brain, and consequent excessive irritation.

On the Non-existence of Sugar in the Blood of Persons labouring under Diabetes Mellitus. In a Letter to Alexander Marcet, *M.D. F.R.S. from* William Hyde Wollaston, *M.D. Sec. R.S.* Read January 24, 1811. [*Phil. Trans.* 1811, *p.* 96.]

Dr. Marcet, having been requested by Dr. Wollaston to examine whether the serous fluid, secreted in consequence of the application of a blister, could be impregnated with prussiate of potash, gave to a young woman five grains of this prussiate, every hour, till she had taken thirteen or fourteen such doses. After the fifth dose, when her urine became blue immediately by addition of sulphate of iron, a blister was applied, and the serum secreted in consequence was collected, whilst her urine still indicated the presence of the prussiate. But in this serum no prussiate could be detected.

Dr. Marcet also repeated Dr. Wollaston's experiment on serum derived directly from the blood, but with this variation, that the blood was drawn by cupping; and he could not discover the presence of any prussiate.

The author observes, that in several instances in which prussiate of potash had been taken by other persons, it could not be detected in their urine. As some of those in whom it failed to appear were taking mercury at the time, he conjectured that the difference might possibly arise from that cause; but as in two other failures no mercury was present, he does not lay much stress on that conjecture.

Reply of Dr. Marcet *on the same Subject.*

This letter contains the details of experiments made several years since.

First. On the serum of blood, with a view to discover some easy means of detecting the presence of sugar added to it.

Secondly. Upon the blood of persons whose urine was known to contain sugar, for the purpose of determining whether it was also present in their blood.

Thirdly. Upon the blood of persons secreting, by urine, other ingredients, which had been swallowed for that purpose, in order to ascer-

tain whether these could be detected in their passage through the blood, from the stomach to the bladder.

The method employed by the author for detecting sugar in the serum, was first to add dilute muriatic acid to the serum, and then to heat it till perfectly coagulated. The water which exudes from healthy serum so coagulated, contains scarcely anything but salts, which crystallize by evaporation. But if a very small proportion of sugar has been previously added, it is immediately detected upon evaporating a drop of this fluid, by the blackness and interruption to the crystallization which are occasioned by it. As a further test of the presence or absence of sugar, a little nitric acid was added to the drop, which in the former case merely occasioned an alteration in the form of the salts, but in the latter a white foam rises round the margin of the drop, and it subsequently turns black.

The author next examined the blood of four persons labouring under diabetes, whose urine contained sugar, and was satisfied that no one of them contained a perceptible quantity of sugar.

Since the formation of sugar did not appear so likely to arise from a new power assumed by the kidneys in diabetes, as from a process of imperfect assimilation by the stomach, and since the possibility of fluids passing from the stomach to the bladder without passing through the blood, had been formerly maintained by Dr. Darwin, it seemed desirable to examine this point by some test more decisive than nitre, which was employed by Dr. Darwin. Dr. Wollaston, therefore, made use of prussiate of potash for this purpose, which he found might be taken without detriment or inconvenience, and could be detected with the utmost facility in the urine by adding solutions of iron. Nevertheless, no perceptible quantity of this prussiate could be discovered in the blood taken from the arm during the secretion of urine highly impregnated with it.

The author also examined other secretions, as the saliva, and the fluid secreted by the nose during a catarrh; but he could not perceive them to be tinged with the prussiate.

He is consequently much inclined to the opinion, of the existence of some channel of conveyance from the stomach to the bladder, not yet rightly understood. For though the agency of an elective power, residing in the nerves as acting cause, may account for the transfer, yet the channel through which they are conveyed remains to be discovered by direct experiments on living animals, which he has not been inclined to undertake.

On the Rectification of the Hyperbola by Means of Two Ellipses; proving that Method to be circuitous, and such as requires much more Calculation than is requisite by an appropriate Theorem: in which Process a new Theorem for the Rectification of that Curve is discovered.
To which are added some further Observations on the Rectification of the Hyperbola: among which the great Advantage of descending Series over ascending Series, in many cases, is clearly shown; and several Methods are given for computing the constant Quantity by which those Series differ from each other. By the Rev. John Hellins, *B.D. F.R.S. and Vicar of Potter's-Pury, in Northamptonshire. Being an Appendix to his former Paper on the Rectification of the Hyperbola, inserted in the Philosophical Transactions for the Year* 1802. *Communicated by* Nevil Maskelyne, *D.D. F.R.S. Astronomer Royal.* Read January 10, 1811. [*Phil. Trans.* 1811, *p.* 110.]

The present communication is designed by the author as an appendix to his former paper on the same subject, printed in our Transactions for 1802.

Although he acknowledges the ingenuity of Mr. John Landen, who devised the rectification of the hyperbola by means of two ellipses, and adds his tribute of applause to that which has been bestowed upon it by the most eminent mathematicians of the Continent, as well as of our own country, Mr. Hellins is nevertheless of opinion, that it is more to be admired as curious than practically useful; since it is circuitous, and requires much more calculation than will be found requisite by the theorem, which is the primary subject of this paper.

Mr. Hellins also adds some further observations on the rectification of the hyperbola, and shows the great advantage of descending series over ascending series, in many cases, and gives several methods of computing the constant quantity by which those series differ from each other.

On a Combination of Oxymuriatic Gas and Oxygen Gas. By Humphry Davy, *Esq. LL.D. Sec. R.S. Prof. Chem. R.I.* Read February 21, 1811. [*Phil. Trans.* 1811, *p.* 155.]

The author, having observed the properties of oxymuriatic gas to be different in consequence of its being prepared in different modes, was endeavouring to determine the nature of these differences, and the causes on which they depend, when he discovered the very singular compound which is the subject of the present paper. For the formation of this compound, he pours a small quantity of dilute muriatic acid upon a large quantity of hyperoxymuriate of potash. A gas is then disengaged, which is capable of being absorbed by water, but may be collected over mercury. It is of a bright yellow colour, approaching to orange, and has nearly the specific gravity of oxymuriatic gas.

It often explodes while collecting, in consequence of heat gene-

rated; and it may always be made to explode by the heat of the hand, with instantaneous extrication of heat and light. After explosion the gas is found to occupy about one sixth part more bulk than before. From the gas so exploded, oxymuriatic gas may be absorbed by water, and there remains about one third part of oxygen.

When copper, or antimony, or mercury, or iron, are exposed to this gas, it has no action upon them till heat is applied; but then they burn with a very brilliant light, and generally with explosion. But charcoal, which has no affinity with oxymuriatic gas, burns only with a dull red light, by union with the diluted oxygen.

Arsenic was acted upon without the application of heat. After a short time it caused an explosion, and united with the oxymuriatic gas. Sulphur caused instant explosion, but was not burned.

Phosphorus caused explosion with brilliant light; and uniting with both constituents, formed phosphoric acid, and solid oxymuriate of phosphorus.

When the gas was mixed with muriatic gas, a gradual diminution of volume took place; oxymuriatic gas was formed, and dew deposited on the sides of the vessel.

These experiments, says Mr. Davy, enable us to explain the contradictory accounts that have been given of the properties of oxymuriatic gas, which have been confounded with those of the explosive compound. That the latter has not been collected before, is principally owing to its being absorbed by water, which has generally been used for receiving the products from hyperoxymuriate of potash; and since water absorbs about ten times its bulk of this gas, nothing could be received in the form of gas but the oxymuriatic, till the water became completely saturated.

The violent explosion, accompanied with heat and light, which is in this instance produced during the separation and expansion of two gases, says Mr. Davy, is a perfectly novel circumstance in chemical philosophy; but he sees nothing in the properties of this gas which is at variance with the conclusions he has before drawn, as to the undecompounded nature of oxymuriatic gas. The weakness of the affinity, with which the constituents are united in it, is, on the contrary, perfectly conformable to the supposition of their belonging to the same class of bodies, and to the idea of their being distinct, though analogous species of matter.

Experiments to prove that Fluids pass directly from the Stomach to the Circulation of the Blood, and from thence into the Cells of the Spleen, the Gall Bladder, and Urinary Bladder, without going through the Thoracic Duct. By Everard Home, *Esq. F.R.S.* Read January 31, 1811. [*Phil. Trans.* 1811, *p.* 163.]

Mr. Home having formerly found that fluids pass from the stomach into the circulation of the blood without going through the thoracic duct, had maintained the spleen to be the channel by which they are

conveyed; but brings forward the present set of experiments to correct that opinion, which he finds to be erroneous.

Mr. Brodie having tied the thoracic duct in some experiments of his own, it occurred to Mr. Home, that under these circumstances the existence or non-existence of any other channel from the stomach into the circulation might be fully established.

A rabbit and a dog were each subjected to this experiment. After tying the thoracic duct, a quantity of infusion of rhubarb was injected into the stomach; and, in an hour after, the urine was examined, and found to be tinged with the rhubarb. In the dog, the bile was also examined, and found to be tinged with the rhubarb. The existence of a channel distinct from the thoracic duct being thus established, the experiment was repeated on a dog whose spleen had been removed four days previously; but still the urine became tinged with the rhubarb, so that the channel is not through the spleen.

As it was possible (though not very probable) that the rhubarb might, by some anastomosis, obtain a passage through the lymphatic vessel, which enters at the angle between the jugular and subclavian veins on the right side, the same experiment was repeated upon another dog, in whom this vessel was also secured by ligature, as well as the thoracic duct, previously to the injection of the rhubarb; but in this experiment also the rhubarb found its way to the bladder, as before. When the spleen of this dog was infused in water, the infusion was slightly tinged with the rhubarb; but when the liver was infused, the proportion of blood present was so great as not to admit of determining whether rhubarb was present or not.

In some of these experiments the thoracic duct was wounded or ruptured, so that chyle was found to have flowed from it, and was collected for experiment: in other experiments the duct itself, the mesenteric glands, and lacteals, were found distended, and the fluid was pressed from them for the same purpose; but it was in no instance found to be tinged with the rhubarb.

These experiments, says Mr. Home, completely establish the fact, that the rhubarb did not pass through the thoracic duct, and also completely overturn the opinion of the spleen being the medium by which it was conveyed. He conceives, therefore, that the rhubarb found in the spleen must previously have entered the circulation, and thence have been deposited, by secretion, in the cells of the spleen. The objection to this opinion is, that there is no excretory duct from the spleen; but Mr. Home observes, that the lymphatic vessels probably perform the office of excretory ducts, as they are both larger and more numerous than in any other organ of the body. In the ass, he remarks, they unite and form one common trunk; and as they terminate in the thoracic duct, it would be a deviation from the general plan of the animal economy if their structure differed from that of other lymphatic vessels.

On the Composition of Zeolite. *By* James Smithson, *Esq. F.R.S.* Read February 7, 1811. [*Phil. Trans.* 1811, *p.* 171.]

Although the substance called Natrolite by Mr. Klaproth has lately been found, under a crystalline form, perfectly similar to that of mesotype, M. Haüy has not thought himself warranted, from this circumstance, to consider these two bodies as of the same species, on account of the difference of their chemical composition; because, according to the analysis of M. Vauquelin, there was not observed to be any soda in zeolite, but, on the contrary, a considerable quantity of lime, which is not a constituent part of natrolite.

Mr. Smithson, on the contrary, is inclined to consider them as the same substance, by the agreement of their chemical properties, as well as crystallographical form. Although he had, many years since, found that the zeolite of Staffa contained soda, as has since been observed by Dr. Hutton and Dr. Kennedy, he still felt uncertain whether these were of the same kind as those analysed by M. Vauquelin: but having lately received from M. Haüy a cluster of mesotype in rectangular prisms, terminated by a quadrangular pyramid, Mr. Smithson took this opportunity of ascertaining whether this substance and natrolite did or did not differ in their composition. The method pursued by the author in the analysis of zeolite was first to expose ten grains of it to a strong heat, for the purpose of ascertaining the loss; and he found it to be ·95 grain. An equal quantity was then dissolved in muriatic acid; and after the solution had been evaporated to dryness, the residuum was exposed to a red heat. Water was then poured upon it, and dissolved a portion, which, upon evaporation, was found to be muriate of soda, weighing 3·15 grains. From the solution of this salt, neither carbonate of ammonia nor oxalic acid occasioned any precipitate; by which it appeared that this zeolite contained no lime.

The residuum from which the muriate of soda had been extracted was next digested in muriatic acid, which dissolved a part, but left undissolved a quantity of silica, which, after being made red hot, weighed 4·9 grains. The muriatic solution was then evaporated to dryness, and exposed to a red heat; and the residuum appeared to be pure alumina. Since it was possible that some magnesia might also be mixed with it, it was dissolved in sulphuric acid, and ignited; but the residuum was not found to yield any sulphate of magnesia by addition of water to it. The whole of the sulphuric acid, however, had not been expelled by heat, but was afterwards separated by nitrate of barytes; and the alumina was estimated to amount to 2·7 grains.

The results thus obtained accorded so nearly with the analysis of natrolite, as given by Klaproth, that it was judged unnecessary to repeat his experiments on that substance.

Mr. Smithson is induced to prefer the name of Natrolite to that of Mesotype, from a desire to preserve unaltered the name given by Baron Cronstadt, the original discoverer of this class of bodies; and

this indeed appears but a feeble tribute of respect to the services which he has rendered to the science of mineralogy.

Since the crystallization of certain mineral substances, in which nothing but earths has been discovered, has appeared problematical to many persons, and has led to the supposition of the existence of unknown acids in their composition, Mr. Smithson endeavours to explain this difficulty, by suggesting that quartz itself may be considered as an acid, to which class of bodies it has analogous qualities: we shall then have a numerous class of silicates, both simple and compound; and zeolite will belong to the latter, and may be regarded as a hydrated silicate of alumina.

Experiments and Observations on the different Modes in which Death is produced by certain vegetable Poisons. By B. C. Brodie, *Esq. F.R.S. Communicated by the Society for promoting the Knowledge of Animal Chemistry.* Read February 21, 1811. [*Phil. Trans.* 1811, *p.* 178.]

The substances selected for these experiments are vegetable poisons only; and they were chosen of the most active kind, that the exact nature of their effects might be more readily discerned. The principal object of the experiments is to determine on which of the vital organs the influence of each poison is exerted, and through what medium the organ becomes affected. The first series of experiments relates to the effects of *internal* application to the tongue and alimentary canal, and the second to the consequences of application to external wounds.

When proof spirit was given to a rabbit in sufficient quantity to kill it, the heart was observed to continue in action after apparent death.

The same observation was made respecting the heart of a cat, killed by injecting the root of aconite into the rectum.

When the oil distilled from bitter almonds was employed, although no more than a single drop had been given to a cat, she died in five minutes. Two drops of the same oil injected into the rectum of another cat, killed it also in five minutes. And the heart, in each instance, continued acting after apparent death.

Distilled oil of tobacco exerted nearly the same energy as the distilled oil of bitter almonds, and apparently in the same way, as the heart was observed to contract after apparent death.

From this circumstance, Mr. Brodie inferred that these poisons exert their primary influence on the brain, and that death ensues in consequence of the suspension of respiration, which is dependent on the brain.

When an *infusion* of tobacco was made use of instead of the *empyreumatic oil*, and injected into the rectum, the effects were different from any of the preceding, as the heart continued to contract, and was uniformly found in a state of extreme distension. Mr. Brodie is, however, of opinion, that the heart was not directly affected, but through the medium of the nervous system. For when the same

infusion was injected into the rectum of a dog whose head had been cut off, and whose respiration was kept up by artificial means, the heart continued to act in the same manner as in the experiments which Mr. Brodie lately communicated to the Society, without being sensibly affected by the infusion.

The author's trials of the *external* application of poisons were confined to the essential oil of bitter almonds, the juice of aconite, and the South American poison called Woorara. They all produced the same effects as the two former had done when applied *internally*, for the heart was observed to contract, as before, long after other symptoms of life had ceased; so that the circulation could be kept up by means of artificial respiration.

With respect to the medium through which poisons affect the brain when they are applied to external wounds, the author's experiments were confined to the woorara. And he endeavoured to determine whether the influence was conveyed by the nerves, or whether the poison itself entered the circulation, either by the absorbents, or through the divided veins. By dividing the nerves of a part, the efficacy of the woorara did not appear diminished, neither did tying up the thoracic duct in any degree interfere with its action. But when a ligature was applied round the leg of a rabbit, so as not to include the sciatic nerve, the rabbit was not in the least affected by the woorara.

The author consequently infers that the woorara acts upon the brain by passing into its substance through the divided vessels of the part to which it is applied.

Since the circulation of an animal could be kept up by an artificial respiration, after the brain had been even completely removed, Mr. Brodie conceived it possible that the functions of the brain might be found to recover from temporary suspension if the circulation were continued for a time by artificial respiration, and that thus the life of the animal might be preserved.

After two experiments, which were not attended with complete success, a third was made upon a rabbit, by applying distilled oil of almonds to a wound in the side. In five minutes it ceased to breathe, and was apparently dead; but by means of artificial respiration continued for sixteen minutes, it was completely restored to life; and on the following day appeared not to have suffered from the experiment.

On the Causes which influence the Direction of the Growth of Roots. By Thomas Andrew Knight, *Esq. F.R.S. In a Letter to the Right Hon. Sir* Joseph Banks, *Bart. K.B. P.R.S.* Read March 7, 1811. [*Phil. Trans.* 1811, *p.* 209.]

In a former paper Mr. Knight showed the influence of gravitation on the plumule and radicle of germinating seeds; in the present he considers the fibrous roots, which, with little comparative regard to gravity, extend themselves in whatever direction the greatest nutriment or moisture is to be found, with an appearance of predilection,

which some naturalists have been disposed to compare to animal instinct. Mr. Knight examines whether this comparison has any foundation.

He remarks, that when trees which require moisture are near to water, by far the greatest part of their roots extend themselves in that direction; but when those to which a dry soil is more congenial are placed in the same situation, their roots take an opposite course. When a tree is placed upon a wall, at a distance from the ground, its powers appear directed exclusively to one object; its roots are extended with well-directed efforts to reach the soil beneath, while its branches are in the mean time stinted in their growth; but as soon as the roots have entered the soil, the branches then grow with vigour and rapidity.

Mr. Knight sowed some seeds of the common carrot and parsnip in light poor loam, under which, at the depth of eighteen inches, he had placed a stratum of very rich mould, incorporated with fresh vegetable matter. The plants grew feebly till the end of summer, but then assumed a very luxuriant growth; and when the roots were examined, they were found nearly cylindrical, with very few fibres emitted into the superficial poor soil, whilst the rich mould beneath was filled with fibres.

When the strata of soil were reversed, the roots were found to have formed accordingly, ramifying, in an unusual manner, into the superficial rich soil, but emitting few of their fibres into the subjacent stratum.

When mould contained in an inverted garden-pot was so supported by frame-work that its under surface was exposed to the air, the radicles of garden-beans in contact with this surface extended horizontally along it, and in a few days emitted many fibrous roots upwards into the mould; but they appear to the author to be guided by laws far more simple than animal instinct.

For the explanation of these laws he refers to the known powers of the alburnum of the stem and branches, by which the sap is propelled, without assistance from the roots, in such proportion as each has power to employ; that which is exposed to the light attracting to itself a larger portion of the ascending sap, for the formation of leaves and of vigorous annual shoots, while those that are shaded are comparatively languid and unhealthy. In the same manner, the author is of opinion, that the stimulus of proper food and moisture in the soil upon the bark of the root may give ability to that organ to attract and employ a larger portion of the descending current of sap. Those roots of a tree upon a wall, that are the worst supplied with moisture, will be first affected with drought, and their points will become rigid and cease to elongate, and the current of descending sap will be employed in the elongation of those more favourably circumstanced, and the tree will appear to have adopted as wise a plan as it could have done if it had possessed the feeling and instinctive powers of animal life.

The cylindrical roots of the carrots and parsnips, in the first experi-

ment, as well as the branched and taper roots of those sown in a soil superficially rich, admit of a similar explanation. So also when the seeds of the bean were placed to vegetate beneath the mould of an inverted pot, the lower surface of the radicles, being exposed to dry air, were rendered rigid and incapable of emitting fibrous roots; while their upper surface in contact with the mould, being preserved in a due state of moisture, emitted fibres in that direction alone in which proper food was to be procured.

In confirmation of this explanation, the author made a corresponding experiment, in which water was so constantly and abundantly supplied, that every part of the radicles was kept equally wet, and then they emitted fibres perfectly obedient to gravitation, without being influenced by the soil above them.

The strength of roots, by which they appear to be wisely adapted to their situations of exposure to the violence of winds, is traced by the author to the operation of another cause, noticed in a former memoir; for the immediate consequence of motion upon the roots, as well as upon the branches, is a deposition of alburnous matter upon the part moved; and hence those roots which immediately join the trunk of an insulated tree become strong and rigid, but diminish rapidly in bulk as they recede from the stem and descend into the ground. But in a sheltered valley, on the contrary, where a tree is protected by its neighbours, and little agitated by winds, the roots grow long, and continue slender like the stem and branches, and hence comparatively much less of alburnous matter is expended beneath the ground.

In the whole of this arrangement the author sees much reason to admire the simplicity of the means employed by the wisdom of nature, but is unable to trace the existence of anything like sensation or intellect in the plants themselves.

On the Solar Eclipse which is said to have been predicted by Thales. *By* Francis Baily, *Esq.* *Communicated by* Humphry Davy, *Esq. Sec. R.S.* Read March 14, 1811. [*Phil. Trans.* 1811, *p.* 220.]

Notwithstanding there may be few facts in ancient history which have given rise to more discussion, this subject still appears to the author to admit of elucidation; for though chronologists have availed themselves of the aid which astronomy could give them in fixing the exact time when this event occurred, and thereby ascertaining the dates of several other events, yet among the various periods assigned for this eclipse by different authors, we find a difference of no less than 43 years between that assigned by Scaliger, who supposed it to have happened on the 1st of October, 583 B.C.; and that supposed by Volney, in his *Chronologie d'Hérodote*, who fixes it on February 3, 626 B.C. The results to which most confidence has been hitherto attached, is, in fact, very nearly the mean between these extremes, and is that preferred by Bayer, in his *Chronologia Scythica*, published in the Petersburgh Memoirs for 1728.

This result was founded on the computations of his friend Mayer, who, by the assistance of the best astronomical tables *then in use*, found that neither the eclipse mentioned by Pliny, Scaliger, Calvisius, Petavius, or Usher, could possibly be the eclipse alluded to by Herodotus. Mayer calculated all the eclipses from 608 to 556 before Christ, and found that of May 603 to be that which best accorded in position and magnitude with that described by Herodotus.

In the Philosophical Transactions for the year 1753, Mr. Costard, by computations similar to those of Mayer, arrived at the same conclusion with respect to this eclipse; but nevertheless, by introducing an allowance for the moon's acceleration, which was not attended to by Mayer, he has assigned a path to the centre of the moon's shadow, which Baily observes does not pass over any part of Asia Minor, and consequently is too much to the southward for the eclipse of Herodotus.

Since the improvements which have been made of late years in astronomy, have shown the tables employed by Mayer and by Costard to be extremely defective, even in respect to the mean motions of the sun and moon, as well as the lunar equations; and since the secular variations derived from the formulæ of M. Laplace were wholly unknown at the time when those tables were constructed, and must have an important effect in determining the place of conjunction at so distant a period, the author has been induced to recalculate the elements of several of these eclipses, from the new *Tables Astronomiques*, published a few years since by the Bureau des Longitudes in France. These calculations, at full length, together with a map containing the paths of the moon's shadow in these eclipses, accompany this paper, for the satisfaction of those who may be interested to enter more minutely into the subject.

The eclipses here calculated are, first, that of Pliny in May, 585 B. C.; next that of Calvisius, 607 B. C., each of which, as well as that of Bayer, passed too much to the south for the eclipse mentioned by Herodotus; while that of Petavius, in July, 597, and that of Usher, September, 601 B. C., passed much too far to the north to have been seen in Asia Minor. With respect to the eclipse of the year 626, suggested by Volney, it was not total, but only annular, and moreover was not visible but to countries far eastward of Asia Minor.

Mr. Baily's inquiries have consequently taken a greater range than those of his predecessors. He has taken the pains to calculate all the solar eclipses from 650 before Christ to 580, and has found only one that was central and total in or near any part of the peninsula of Asia Minor.

This eclipse took place on the 30th of September, 610. The centre of the moon's shadow, in this instance, passed in the forenoon of that day, in a straight line, over the north-eastern part of Asia Minor, through Armenia into Persia, where the sun was centrally eclipsed on the meridian. The path of the moon's shadow is estimated by the author to have passed over the very mouth of the river Halys,

and to have crossed the very ground where the armies of Cyaxares and Alyattes probably met. And if the order of events belonging to the reign of Cyaxares, as related by Herodotus, does not accord so exactly as might be wished with this determination, and cannot be entirely reconciled to their dates, Mr. Baily would attribute the confusion to the want of authentic documents at the time the history was written.

Although the author has employed in these calculations the secular variations of the moon's mean longitude, mean anomaly, and mean distance from her node, as deduced from the formulæ of Laplace, and given in the *Tables Astronomiques,* he expresses some doubts of the accuracy of these results; and his doubts are founded upon an eclipse recorded by Diodorus to have happened during the voyage of Agathocles from Syracuse to invade Africa, in the year 310 B. C., in which, when computed according to the present tables, the path of the moon's shadow appears to have passed so much more south than Agathocles can be supposed to have been at that time, that the latitude of the moon would require to be at least 3° greater than our present tables make it.

These observations (if correct) would show the necessity of some alteration of the secular variation of the moon's mean distance from her node; but this hypothesis, Mr. Baily observes, could not be reconciled with the eclipse mentioned by Herodotus; for by means of a corresponding correction, the eclipse of 610 would be found not to be total to any part of Asia Minor; and there is no other that could possibly be central and total within the utmost limits that are reconcileable with any received systems of chronology.

An Account of the great Derbyshire Denudation. By Mr. J. Farey, Sen. *In a Letter to the Right Hon. Sir* Joseph Banks, *Bart. K.B. P.R.S.* Read March 21, 1811. [*Phil. Trans.* 1811, *p.* 242.]

It is now well known, says Mr. Farey, to many observers in geology, that the clay strata on which the metropolis is situated, extend north-eastward through Essex, Suffolk, and Norfolk, and are incumbent on the great chalk stratum, which reaches from the Isle of Wight to Flamboro'-head; and that these, as well as many subjacent strata that are known, dip in general to the south-east, and basset out, or appear at the surface in succession, to any one travelling toward the N.W., until he has passed certain strata of lias, clay, and sand. Beneath these, says the author, are found marks of an immense stratum of red marl, which seems (to him) to have extended over all the remainder of the British islands. In this stratum are contained local strata of gypsum, rock salt, sand, micaceous gritstone, &c.: to this stratum also, according to the author, belong the great nodules of slate, green stone, sienite, basalt, &c. that form hills or mountains, intersected by mineral veins, in the western parts of the kingdom. In many parts, however, the red marl itself is no longer found; but instead of it various strata, subjacent to it, have

been elevated to a considerable height, and subsequently denudated by the operation of water, as observed by Dr. Richardson, in his remarks upon the basaltic counties of Antrim and Derry in Ireland.

It is to these tracts of elevated strata, and their abrupt terminations, or *faults*, in the county of Derby, that the author's observations have been principally directed; and he enumerates a series of *four* different elevations, in succession, one within the other, of which the innermost is most elevated. The outermost, or least elevated, extends from Nottingham northward into Yorkshire, as far as the river Wharf, this being the eastern boundary of the tract; while its western boundary extends from near Stone, in Staffordshire, to the neighbourhood of Manchester. On the south it is bounded by a fault, which the author calls the great Derbyshire fault; but its northern extent has not yet been ascertained by actual observation.

The second tract, which is much more elevated than the preceding, is separated from it by a fault, which, from the irregularity of its course, is termed the zigzag fault. The elevation of this second tract, on its southern extremity, is such as to bring the great limestone shale, which underlies all the coal strata, against the red marl at the surface.

The third inner tract is considered by the author as still 400 yards more elevated than the second; so that the fourth or lowest limestone rock is raised into a high hill, with red marl at the foot of it, on the other side of the great Derbyshire fault, which here occasions a derangement far exceeding anything that has hitherto been conceived to exist.

Of these tracts the outermost appears to have but little inclination to the horizon; but the second and third are inclined to each other, and to the adjacent strata, in a direction from S.W. to N.E., the mutual intersection of the strata, or hinge on which they may be supposed to turn, passing from Cromford in a north-westerly direction.

The fourth tract includes Bakewell, and a small district round it, and is surrounded by a fault, which Mr. Farey denominates the Bakewell fault. The western side of this tract is most elevated, as well as that of the third, so as to occasion a great elevation of some strata of toadstone, in situations where their appearance had not before been explained. It is in this district that, in Mr. Farey's estimation, the lowest strata anywhere observable in Britain appear. He reckons as many as three distinct series of coal measures, separated by thick strata of limestone, and of red marl, similar to that which intervenes between the lias and the uppermost stratum of coal; and the lowest of the coal measures is that of the coal-field of Derbyshire, Nottinghamshire, and Yorkshire, lying beneath the yellow limestone rock. Beneath these follows what is called the fourth limestone rock, which extends from Castleton, in Derbyshire, southward to Weaver-hills, near Wootton and Ramsor in Staffordshire. This he supposes to be the lowest of all British strata; and to this circumstance ascribes the occurrence of some phenomena, which are said to appear nowhere else.

An Account of an Appendix to the small Intestines of Birds. By James Macartney, *Esq. F.R.S.* Read March 21, 1811. [*Phil. Trans.* 1811, *p.* 257.]

Although almost every author who has written upon the incubation of the egg has observed the direct communication between the yolk-bag and the small intestines of the chick, and although some of them have observed that this duct remains in the form of a small cæcum during life, it appears to have escaped the notice of any one, that in some species of birds this part is of considerable size, and possesses a structure peculiar to itself.

It was in the snipe that Mr. Macartney first observed its magnitude to exceed that of the cæca of the great intestines; and he has since found that in the woodcock and curlew it is proportionally large. In the black coot also, it is long, but slender, like the rest of the intestines in that bird.

In the swan and goose it does not bear the same proportion as in the preceding, though somewhat larger than in the generality of birds.

This cæcum consists always of two tunics, corresponding to the peritoneal and villous coats of the intestinal canal in general, but without any appearance of intervening muscle. Its interior surface is composed of small cells, like the assemblage of mucous follicles, found in various parts of the alimentary canal. The matter contained in this appendix has never been found the same, as in the adjoining intestines, but it has been always filled with a mucous fluid, which it seems to secrete.

This appendix, in most birds, retains evidence of its origin; for the remnant of the yolk-bag is commonly found adhering to its extremity, and still communicates freely with it, especially in the Accipitres, and in the passerine tribe. In the nightingale Mr. Macartney observes, the duct is scarcely visible; but the yolk-bag remains during life as a sac, the size of a large pea, communicating with the intestine.

The preceding facts, says the author, are curious instances of an organ of fœtal life retained in the full-grown bird, for the exercise of a particular function.

An Account of a vegetable Wax from Brazil. By William Thomas Brande, *Esq. F.R.S.* Read May 9, 1811. [*Phil. Trans.* 1811, *p.* 261.]

The substance here examined by Mr. Brande, had been sent to Lord Grenville from Rio de Janeiro, and by him given to Sir Joseph Banks, in the hope that when its properties were investigated, it might be found to answer the purposes of bees' wax, and become a valuable article of commerce between Brazil and this country.

It is said to be the produce of a tree of slow growth, called by the natives Carnauba, and growing in the most northerly part of the

Portuguese settlements; and the same tree also produces a gum as food for man, and another substance employed to fatten poultry.

The substance examined by Mr. Brande resembles that procured from the *Ceroxylon Andicola* described by Humboldt. The Ceroxylon, however, is a tall palm-tree, growing about 1000 toises or more above the level of the sea; but the Brazilian plant is a low tree, and grows in a country where it does not appear that there are any mountains. By analysis also, these products differ entirely; for that of Humboldt, according to Vauquelin's analysis, consists of two thirds resin and one third wax; while that from Brazil is entirely wax, without any perceptible quantity of resin.

Mr. Brande received this wax in the state of a coarse pale-grey powder, soft to the touch, mixed with pieces of bark, and other impurities, amounting to nearly 40 per cent.

It melts at 206° Fahrenheit; and if then purified by straining through linen, it acquires a dirty green colour; when cold it is hard and brittle, and its specific gravity is 980.

In water this wax is wholly insoluble; but by boiling some hours, it communicates a brownish hue to the water, and its peculiar smell.

Alcohol also has no effect on it when cold. Nevertheless, by the assistance of heat, two fluid ounces dissolve ten grains; but eight of these are deposited again as the solution becomes cool, and the remainder may be precipitated by addition of water.

Ether dissolves a small proportion at the temperature of 60°; and when boiled upon it, two fluid ounces dissolve thirty grains, but deposit twenty-six upon cooling.

Fixed oils unite with it very readily by the assistance of heat, and the compounds are in general perfectly soluble in ether, which appears owing to the solubility of these oils, although they are not generally supposed to be soluble.

According to Mr. Brande's experiments, four fluid ounces of ether dissolve $1\frac{1}{4}$ fluid ounce of oil of almonds, $1\frac{1}{2}$ fluid ounce of olive oil, $2\frac{1}{2}$ fluid ounces of linseed oil; and with castor oil it unites in any proportion.

In alcohol, having the specific gravity of 820, castor oil is also perfectly soluble; but the other fixed oils are very sparingly soluble; and even castor oil is scarcely soluble in alcohol that has the specific gravity of 840; unless camphor be added to it. But even this addition will not render other fixed oils soluble in the same menstruum.

When the wax was boiled in a solution of caustic potash, it communicated to the liquor a slight rose colour, but was not dissolved, nor altered in its properties.

The effects of a solution of pure soda were precisely similar, but pure ammonia had scarely any effect on the wax.

By boiling in nitric acid, the colour of the wax is converted into a deep yellow; and after being washed and cooled, it is found to have become more brittle and harder than before; but neither the fusibility nor the inflammability of the wax are impaired, even by the alternate action of acids and of alkalies upon it.

If the wax be spread thin upon glass and exposed to the action of light, its colour is rendered paler; but the author has not yet succeeded in bleaching it effectually.

Sulphuric acid changes the colour of the wax to a pale brown; and if heat be applied, the decomposition usual with such substances takes place, with extrication of sulphurous acid gas, and decomposition of charcoal.

Though cold acetic acid has very little action on it, yet when boiled upon it for a very long time, it appeared to have rendered it nearly white; but when the wax was afterwards washed with water and fused, it resumed its former colour.

When it is fused in oxymuriatic gas, muriatic acid and water are formed, and charcoal is deposited.

Though Mr. Brande's attempts to bleach this wax have not hitherto completely succeeded, yet since it has been rendered nearly white by the usual method of exposure to light after the action of nitric acid upon it, there seems no reason to suppose that it might not be perfectly bleached by exposure for a sufficient length of time.

With respect to its combustion in the form of candles, the author's trials have been very satisfactory. When the wick is duly proportioned, the combustion is as perfect and uniform as that of any other wax; and when about one sixth or one tenth of tallow is added to it, the brittleness which this wax has in its natural state is removed, without communicating to it any unpleasant odour, or impairing the brilliancy of the flame.

Astronomical Observations relating to the Construction of the Heavens, arranged for the Purpose of a critical Examination, the Result of which appears to throw some new Light upon the Organization of the celestial Bodies. By William Herschel, *LL.D. F.R.S.* Read June 20, 1811. [*Phil. Trans.* 1811, *p.* 269.]

Dr. Herschel, having repeatedly re-examined many of the nebulous appearances which he has formerly described, finds that these objects may be arranged in a certain order of regular succession, so as to be viewed in a new light; and in consequence of these views, his opinions respecting the inferences to be drawn from the phenomena have undergone a gradual change, especially with regard to the possibility of resolving many of them into stars, as he had formerly supposed might be done by telescopes of higher power than he had yet employed.

His present arrangement begins with the appearances of diffused nebulosity. Of these he has determined the positions and magnitudes of 52 in number, the aggregate extent of which amounts to 150 square degrees; and since the depth of each may be supposed nearly equal to the length or breadth, the total amount of luminous matter contained in the small proportion of infinite space which we see, exclusive of that which is too dilute to be visible, exceeds all calculation.

Of these nebulous diffusions, the intensity of the light is not

uniform; for though in some parts their light is uniformly milky, in others it is more faint, and might, at first sight, be thought resolvable into stars. The greater brightness in one part than in another may arise either from greater depth or from greater density, and is thought by Dr. Herschel rather to arise from the latter, which he ascribes to a mutual gravitation of its parts to each other. Facts, says the author, are not wanting to prove that such a power has been exerted; and it is his intention to point out a series of phenomena where the vestiges of such exertion may be distinctly seen.

Those nebulæ, which from their present want of uniformity seem to have more than one centre of attraction, may, in fact, at this time consist of two nebulæ, the limits of which unite only in appearance; or they may be actually united, but undergoing a gradual separation.

In the same manner other nebulæ, for which the author refers to his catalogues, are found to be treble, quadruple, sextuple, &c.

With regard to the forms of nebulæ, some are narrow and very long, others of equal length are in a certain degree broader; others again are of an irregular figure; and from all these, nothing can be inferred as to their extent in the third dimension, which is not seen. But when the visible surface is nearly elliptical, and still more if the circumference be nearly circular there, a spheroidical or spherical form may with some confidence be presumed; and these, it is observed, are forms naturally resulting from a principle of attraction.

The central brightness of some nebulæ point out the seat of principal attraction; and though in many such a difference is scarcely perceptible, there are many others in which the brightness of the centre is considerably greater than that of their circumference, so as to present very much the appearance of many telescopic comets, of which the nucleus is not distinct; and others also, in which a central nucleus is distinctly seen, surrounded by a bright and circular nebula.

These various degrees of condensation, seen in different nebulæ, are supposed by the author successively to take place in the same nebula, and are thus connected into one view, to show the progress of condensation.

The condensation, says Dr. Herschel, may be considered as evidence of their materiality; and he also infers opacity, which is another property of matter, from the circumstance of their not being so bright as he would expect from their observed condensation.

The appearance of certain very regular nebulæ, with extensive branches, suggests to Dr. Herschel various queries respecting the gradual change and ultimate result of condensation.

Do not the branches connected with a nucleus resemble the zodiacal light connected with our sun?

May not portions of branches collect into a planetary form and revolve around the central nucleus, having themselves a rotatory motion, in consequence of the inequality and irregular position of different branches?

Among the nebulæ that have been formerly observed by the author, he refers to seven, which he considers as having approached very near to final condensation; and of these he observes, that we see only a superficial lustre, resembling that of planets, which are opake bodies, and not such as might be expected if the nebulous matter had no other quality than that of shining, and were perfectly transparent.

The author observes, that the spheroidal form which prevails among nebulæ, is another circumstance of resemblance to planetary bodies, from which also their rotation on their axes may be inferred.

That nebulæ do really undergo successive changes, the author deduces not only from a comparison of different nebulæ with each other, but from a comparison of his own observations, made upon the nebula in Orion at this time, with those which he himself made thirty-seven years since.

The figure of it, at that time, he also then observed to differ from that given by Dr. Smith in his optics; and he now remarks, that it differs from the delineation given by Huygens in his *Systema Saturnium*.

In the course of the gradual contraction of this nebula, Dr. Herschel has also observed certain stars which had appeared nebulous to become distinct by removal of a nebulous covering, and thence infers that this nebula is certainly nearer to us than stars of the seventh or eighth magnitude, and possibly not more distant than those of the third.

Notwithstanding, therefore, the extreme dissimilitude between the appearance of diffused nebulosity and that of a star, they seem to have a natural connexion by the several intermediate gradations that have been described. A nebulosity may be conceived so dilute as to be invisible till partially condensed; a nebulosity may become converted into a planetary nebula. The planetary nebula with uniform light may, by gradual condensation of its centre, be converted into a stellar nebula with bright central nucleus, surrounded by a more dilute bur; and this at last assumes the appearance of a complete star, by condensation of so large a portion of its nebulous matter, that the remainder is no longer visible by the best telescopes.

Experiments to ascertain the State in which Spirit exists in fermented Liquors: with a Table exhibiting the relative Proportion of pure Alcohol contained in several Kinds of Wine and some other Liquors. By William Thomas Brande, *Esq. F.R.S.* Read June 13, 1811. [*Phil. Trans.* 1811, *p.* 337.]

An opinion having been entertained by many persons, that alcohol which has been distilled from wine does not exist ready formed in the liquor, but is generated during the process of distillation, Mr. Brande undertook a repetition of Fabroni's experiment, on which this opinion is principally founded; but when he added four ounces of dry subcarbonate of potash to eight fluid ounces of port, no alcohol was separated, although some of the same wine had previously been

ascertained to yield one fifth part of alcohol by distillation. When the same experiment was repeated on the same wine, to which one seventh part of alcohol had been previously added, still none was separated by subcarbonate of potash: but when so much as one third part had been added, then a very small proportion was found to float upon the surface after it had stood twenty-four hours.

When madeira or sherry were employed instead of port, the results were nearly the same. Since the method of Fabroni failed of detecting the presence of alcohol, unless the quantity was very considerable, it became necessary to have recourse to some other method of proving or disproving the presence of it as a product of fermentation; and Mr. Brande conceived, that if it were formed by the heat applied in distillation, the quantity should in that case be different when the same liquor was distilled at different temperatures.

In the first of four processes of distillation, port wine was made to acquire the heat of 200° by addition of muriate of lime, and one half was quickly distilled over; and in the last, an equal quantity of port was kept for five days at the temperature of 180°, till half the quantity had passed over into the receiver; but in all these experiments the specific gravities of the products were so nearly the same, that there did not appear to be any difference in the quantity of alcohol obtained.

Mr. Brande also attempted to separate alcohol from different kinds of wine by freezing; but the cake of ice produced was spongy, and would not allow any portion of alcohol to separate from it.

The author having thus, to his satisfaction, proved the existence of alcohol ready formed in fermented liquors, undertook, in the next place, to ascertain the relative strength of different kinds of wine; and he concludes the present communication with a Table, in which is expressed the proportion per cent. of alcohol contained in a given measure of the several liquors that he has examined.

In this table the alcohol obtained from Port varies from 21 to nearly 26 per cent.; Madeira 19 to 24; Sherry not so much as 20 per cent.; Claret from 13 to 16; Lisbon 19; Marsala nearly 26; Champagne from 11 to 13; Burgundy 12 to 14; Hock 9 to 14; Raisin wine 25¾; Currant wine 20½; Cider and Perry nearly 10; Ale nearly 9 per cent.; good rum and brandy containing 53 per cent. of alcohol at the same standard of ·825 specific gravity.

Account of a Lithological Survey of Schehallien, made in order to determine the specific Gravity of the Rocks which compose that Mountain. By John Playfair, *Esq. F.R.S.* Read June 27, 1811. [*Phil. Trans.* 1811, *p.* 347.]

Notwithstanding the skill with which Dr. Maskelyne conducted the astronomical observations upon Schehallien, and the accuracy with which he may be presumed to have measured the deflection of his plumb-line from the perpendicular, whereby he discovered the actual attraction of that mountain; and although great ingenuity

was manifested by Dr. Hutton in deducing from thence the mean density of the earth; there remained one source of uncertainty in their results, dependent on the specific gravity of the mountain, which they assumed to be 2·5, but were aware that this might not be so accurate as would be desirable in a standard to which the density of the earth is directly compared.

It was for this purpose that Mr. Playfair, with the assistance of Lord Webb Seymour, undertook the present survey of Schehallien, ascertaining, as accurately as they were able, the direction and inclination of the strata of which the mountain consists, and collecting specimens of all the varieties of rock which they could discover, in order to determine the specific gravity of each by direct experiment, and thence to estimate the correction which it might be necessary to introduce into Dr. Hutton's calculation.

Schehallien, like all other mountains in its vicinity, was found to consist entirely of primitive rocks in strata, which stretch in a direction from S.E. to N.W., and nearly vertical in position; but in some parts towards the base, they deviate as much as 15° from the perpendicular. The most elevated and by far the largest part of the mountain was found to consist of granular quartz, extremely hard, compact, and homogeneous, of which the specific gravity, by an average of thirteen specimens, was nearly 2·64. Adjacent to this mass, on each side, and next in quantity, is a micaceous schist, containing hornblende; and lower down toward the base appear strata of granular limestone, highly crystallized, and containing mica. The specific gravity of this part of the mountain, on an average of fifteen specimens, was found to be 2·81.

Beside these there are also veins or dykes of porphyry and greenstone, which intersect the other strata at right angles, but bear a very small proportion to the general mass of the mountain.

With respect to the micaceous schist and limestone, which are distinctly stratified in vertical planes, their continuity from one extremity of the mountain to the other may with confidence be presumed; but this is not the case with respect to the granular quartz, which may either be interposed between them, and continue with them to an indefinite extent in length and depth, or may be of subsequent formation, occupying only the summit of the mountain, and not penetrating into the interior.

Since the result of the calculation of the earth's density is materially affected by the difference of these suppositions, the author has made an estimate according to each hypothesis.

With the exception of the corrections thus obtained for the specific gravity of the mountain, Mr. Playfair adopted the method employed by Dr. Hutton, of dividing the whole mass of the mountain into a certain number of vertical columns, formed by cylindrical sections, so that their respective attractions on the plumb-line could with great facility be computed upon the supposition of uniform density throughout.

Upon the former of the two hypotheses of Mr. Playfair, in which

the granular quartz is supposed to pervade the base of the mountain to an indefinite depth, the corrections, in proportion to the mere difference between the specific gravities assumed by Dr. Hutton and that found by experiment, were simple in comparison to those necessary on the second hypothesis; but in each case the attractions of opposite portions of the cylindrical sections became, in fact, unequal, and consequently required to be computed with due regard to their respective specific gravities, as well as the azimuth and altitude of each.

The nature of these computations is fully explained by the author, but cannot be understood without reference to the figures which accompany them. The resulting density deduced according to the first hypothesis is found to be nearly 4·56; that deduced by Dr. Hutton having been only 4·48: but according to the second hypothesis, the difference is far more considerable; the density in this case being 4·87.

This last is nearly a mean between that of Dr. Hutton and the density of 5·48, which Mr. Cavendish inferred from a very different mode of investigation.

By considering the experiments on Schehallien alone, the density of the earth might be assumed to be between the limits above assigned, the mean of which amounts to 4·71. Mr. Playfair, however, thinks it desirable that an element so important in physical astronomy should be the result of many experiments, and recommends the selection of granite mountains, if possible, for this purpose, because their homogeneity might be presumed with considerable confidence.

Observations and Experiments on Vision. By William Charles Wells, *M.D. F.R.S.* Read July 4, 1811. [*Phil. Trans.* 1811, *p.* 378.]

The experiments here described by Dr. Wells were made in consequence of an imperfection of sight occurring to his observation, which he has nowhere seen upon record. The subject to whom the disorder happened was a gentleman about thirty-five years of age, who, after a slight catarrh, had been seized with a degree of stupor, and weight of his forehead, accompanied with a paralytic state of the right eyelid. The pupil of this eye was also observed to be much dilated; and he had lost all power of adapting that eye to near objects, although he could see at a distance with great distinctness. The left eye also became shortly after affected in a slight degree, and in a similar manner.

The nature of the defect became evident by trial of spectacles; for it was found that convex glasses enabled him to read with perfect ease, and supplied the want of the power of adaptation which he had before possessed.

Since the application of the juice of belladonna to the eye occasions a dilatation of the pupil, it appeared to Dr. Wells not improbable that it might also affect other muscular powers of the eye. It was his intention to have made this experiment on his own eyes:

but he found their power of adaptation too far diminished by age for such a trial. He however prevailed on Dr. Cutting, a young physician of his acquaintance, to make trial of it. The result was perfectly conformable to the supposition. The dilatation of the pupil, it is true, commenced sooner than any other affection of the eye; but in the course of three quarters of an hour, the eye, which before the experiment could see at six inches, could not now see at less than three feet and a half: and when its pupil had acquired the greatest dilatation, the rays from a candle, even at eight feet distance, could not be made to converge on the retina, but only those from stars, or from very distant lamps. The defect thus occasioned by belladonna was found nearly in the same state on the following day; and it was not till the ninth day that the power of adapting the eye to near objects was completely restored. During the whole of this time it was observed that the affection was wholly confined to the left eye, on which the experiment was made, and that the right eye remained unaltered; and in the same manner, when the experiment was afterwards repeated on the right, the left was then wholly unaffected by the belladonna.

The next observations relate to the changes which naturally take place in different eyes by age. With respect to those who are short-sighted, it has been generally asserted by systematic writers, and generally believed by others, that their eyes are rendered fitter for seeing distant objects; but Dr. Wells has observed, in various instances, that this was not the case.

One gentleman, a fellow of this Society, who was short-sighted in early life, and consequently in the habit of using spectacles with concave glasses constantly, could see with them perfectly at a great variety of distances till he arrived at the age of fifty. But he then began to observe that distant objects viewed through the glasses to which he had been accustomed, were indistinct; and he found it necessary to use others which were more concave for seeing objects at great distances. But along with this change of his sight, another occurred of an opposite kind: for he now found, that when he wished to examine minute objects attentively, it was necessary to remove his spectacles entirely, and employ the naked eye alone. It was true, therefore, that, with respect to near objects, he *had* become longer-sighted, but in fact his range of vision was shortened equally at the opposite extreme, so that the mean is little altered from what it always has been.

In a second instance the variation produced by age in a short-sighted person was the same in kind, but not hitherto in so great degree.

In a few trials which Dr. Wells has made upon short-sighted persons with belladonna, the diminution of the range of adaptation has not taken place at both extremities, but the power of seeing near objects has alone been diminished.

He is not, however, altogether satisfied with these experiments; and designs to pursue them further, and at some future time to communicate the results to the Society.

In the course of these experiments Dr. Wells observes, that the sympathy between the eyes, which is in general considered as sympathy of the iris, is in fact sympathy of the retina; for when the pupil of one eye is dilated by belladonna, the pupil of the other becomes so much the more contracted, in consequence of the greater light which the enlarged pupil admits.

He remarks, also, that though he has lost, in great measure, the power of adaptation, he has in no degree lost any command of the external muscles, but can make the optic axes meet at any short distance from his face, to which he could formerly make them converge. So also, while Dr. Cutting's eyes were under the influence of belladonna, the powers of the external muscles remained unimpaired; whence it appears, that the power of adapting the eye to different distances is not dependent on the external muscles, but rather to be referred to the crystalline lens, although the muscularity of that organ does not appear to Dr. Wells to be by any means established.

On the Grounds of the Method which Laplace *has given in the second Chapter of the third Book of his* Mécanique Céleste *for computing the Attractions of Spheroids of every Description. By* James Ivory, *A.M. Communicated by* Henry Brougham, *Esq. F.R.S.* Read July 4, 1811. [*Phil. Trans.* 1812, *p.* 1.]

Sir Isaac Newton, who first considered the figure of the earth and planets, confined his view to the supposition of their having been originally in a fluid state; and he conceived them to retain the same figure which they assumed in their primitive condition; and those mathematicians who succeeded him in the same path of inquiry have seldom ventured beyond this limited hypothesis, and have shown, that when a body composed of one uniform fluid revolves about its axis, or even if it consists of several fluids of different densities, its parts will be in equilibrium, and it will preserve its figure when it has the form of an elliptic spheroid of revolution oblate at the poles.

But though the supposition of original fluidity of the mass simplifies the investigation, it does not seem to be warranted by what we see of the surface; for in that case, Mr. Ivory observes, the arrangement of all the heterogeneous matters would have been according to their densities; those least dense occupying the surface with gradual increase of density to the centre; whereas, on the contrary, nothing can be more irregular than the density of such solid parts of the earth as come under our observation, and the elevation of continents above the level of the sea, as well as the depths of the different channels which contain the waters of the ocean.

Moreover, according to the latest and best observations made for the express purpose of determining the figure of the earth, it does not appear to be of any regular elliptic form.

Since the hypothesis of Newton is, therefore, not consonant to observation, it became necessary to consider the subject in a more

enlarged point of view; and D'Alembert has extended his researches to other figures beside the elliptic spheroid, and has invented a method of investigating the attractive force of a body of any proposed figure, and composed of strata, varying in density according to any given law; but his method, though ingenious, is destitute of the requisite simplicity.

Laplace has also treated this extremely difficult question with his usual skill, and has deduced the relation between the radius of the spheroid and the series for the attractive force, upon a point without or within the surface, in a manner admirably simple when the complicated nature of the question is considered.

In the course of his investigation, Laplace lays down a theorem, which he affirms is true at the surfaces of all spheroids that differ but little from spheres. This proposition is enunciated in the *Mécanique Céleste* in the most general manner, comprehending every case in which the attractive force is proportional to any power of the distance between the attracting particles. But the demonstration which Laplace has given of this proposition appears to Mr. Ivory not to be conclusive. It is, says he, defective and erroneous, because a part of the analytical expression is omitted without examination, and is rejected as evanescent in all cases; whereas it is so only in particular spheroids, the radii of which are expressed by rational and integral functions of a point in the surface of a sphere; and though the quantities which Laplace has omitted are then really equal to nothing, yet, says Mr. Ivory, this does not happen for any reason assigned by Laplace, but for a reason that has no manner of connexion with anything touched upon in his demonstration.

In order to avoid all discussions which are not of real use to the inquiry into the figures of the planets, Mr. Ivory confines his attention chiefly to the case of nature, in which attraction follows the law of the inverse proportion of the squares of the distances. But he does also briefly examine the theorem of Laplace, in the general sense in which it is laid down in the *Mécanique Céleste*; and he admits, that when the exponent of the law of attraction is positive, and not less than unity, then the demonstration of Laplace is not liable to so much objection, and the theorem is in that case true to the full extent of his enunciation; but he observes, that when the exponent is negative, then certain quantities become infinitely great, instead of being equal to nothing, as the theorem of Laplace would require them to be.

The writings of no author on any subject, says Mr. Ivory, are entitled to more respect than those of Laplace on the subject of physical astronomy; and, consequently, it was not till after the most mature reflection that he has ventured to dissent from an authority for which he has the utmost deference. But in a work of so great extent as the *Mécanique Céleste*, which treats of so great variety of subjects, all very difficult and abstruse, it could hardly be expected that no slips or inadvertencies have been admitted, even by an author whose knowledge of the subject he treats is so profound, and the

correctness of whose views is established by so many important discoveries.

On the Attractions of an extensive Class of Spheroids. By James Ivory, *A.M. Communicated by* Henry Brougham, *Esq. F.R.S.* Read November 14, 1811. [*Phil. Trans.* 1812, *p.* 46.]

In his second paper, Mr. Ivory investigates the attractions of that particular class of spheroids mentioned in the former; for though it is to these that the theorems of Laplace may strictly be applied, it is liable to the important objection, that the terms of his series near the beginning cannot be found without previously computing all the rest. The analysis of Mr. Ivory, on the contrary, is direct; and every term of his series is deduced directly from the radius of the spheroid.

In an appendix to these papers, Mr. Ivory adds some remarks upon a memoir of Lagrange, upon the same subject, published at Paris in December 1809, but which had not till lately been received in this country.

An Account of some Peculiarities in the Structure of the Organ of Hearing in the Balæna Mysticetus of Linnæus. *By* Everard Home, *Esq. F.R.S.* Read December 12, 1811. [*Phil. Trans.* 1812, *p.* 83.]

From the time that Mr. Home discovered the muscular structure of the membrana tympani in the elephant, he has been seeking an opportunity of prosecuting the same inquiry on a similar scale, by examining the ear of a whale, and has at length succeeded in procuring the head of a young whale preserved in brine. As the skin had been taken off, a portion of the meatus externus had been removed along with it; but it did not appear that much was lost, as the outward extremity partook of the dark colour of the outer skin of the head. This passage was 5½ inches in length, and only one fourth of an inch in diameter; but near the tympanum it widened to about 1¼ inch, and this is the breadth of the membrana tympani itself.

This membrane, instead of being concave externally as usual, is convex, so as to project nearly an inch into the meatus externus. The membrane consists of four parts: first, a cuticular covering, next a strong membrane, then a layer of muscular fibres; and lastly, another membranous lining towards the tympanum. It is remarkable that this membrane has no connexion whatever with the handle of the malleus, as in other animals.

The cavity of the tympanum is of an oval shape, capable of containing a pint of fluid, surrounded by the concave surface of a large bone peculiar to the whale, detached from the skull, and having only a loose connexion with the petrose portion of the temporal bone. This cavity terminates, as usual, in the eustachian tube, which is 2¼ inches long, terminating by a small aperture, having a valvular structure, and opening into the canal leading to the blow-hole.

Within the cavity of the tympanum is a part peculiar to the whale. This is a membranous fold, or broad ligament, stretched across the cavity, having the form of a triangle, or rather the sector of a circle, the apex of which is attached to the short handle of the malleus, having one side detached, and passing across the centre of the membrana tympani, and its base attached to the concave surface of the hollow bone, at a small distance from the bony rim to which that membrane is connected.

The long handle of the malleus has no connexion with any other part; but the forms of this bone, of the incus, and stapes are much the same as in the human ear; there being no considerable difference excepting in the want of the os orbiculare.

The vestibulum, semicircular canals, cochlea, &c., differ in nothing material from the usual construction of these parts.

From this structure it appears to the author that the membrana tympani, which is subject in the whale to vast differences of pressure from without, is not well fitted, under all circumstances, to convey the nicer vibrations of sound to the ossicula auditûs, but that the membrane which projects across the cavity, being exposed to the same medium on both sides, will freely continue, and communicate the impressions it receives, unaffected by any differences of pressure.

Chemical Researches on the Blood, and some other Animal Fluids. By William Thomas Brande, *Esq. F.R S. Communicated to the Society for the Improvement of Animal Chemistry, and by them to the Royal Society.* Read November 21, 1811. [*Phil. Trans.* 1812, *p.* 90.]

The author, after referring to those authorities by which he had been misled into the supposition that the colour of the blood depended on the presence of iron, until he had tried how slight effect it produced by infusion of galls, proceeds to a series of experiments which he has made upon chyle and on lymph, for the purpose of comparing their composition with that of blood, the examination of which is divided into three sections, in which he treats separately of the serum, the coagulum, and the colouring matter.

The chyle employed in these analyses was collected by Mr. Brande while assisting Mr. Home and Mr. Brodie in their experiments on different animals; attention being always paid to the interval that had elapsed since the last meal; upon which circumstance its qualities were found to depend more than upon the animal from which it was taken. About four hours after a meal, the chyle is supposed to be in its most perfect state, and is then uniformly white, like milk. At longer periods it becomes more dilute, like milk and water, till at length, when an animal has fasted twenty-four hours, the fluid contained in the thoracic duct is reduced to the state of mere lymph.

The taste of chyle is rather salt, with a degree of sweetness, and, by the test of violets, appears very slightly alkaline. In about ten minutes after removal from the thoracic duct, it coagulates, and ulti-

mately separates into two parts, as blood does, exhibiting a firm coagulum surrounded by a transparent colourless fluid. The former has more the properties of cheese obtained from milk, than of the fibrine of the blood; while the serous part also is like whey, and contains a species of sugar which at least very nearly resembles the sugar of milk.

The next subject which Mr. Brande undertakes to examine, is the lymph found in the thoracic duct of animals deprived of food for twenty-four hours before death. This is rendered slightly turbid by alcohol, but is not coagulated either by heat or by acids. It produces no change in vegetable blue colours till evaporated nearly to dryness. After incineration, it is found to contain a small portion of common salt, but no indications of iron.

The serum of blood has been so frequently examined, that Mr. Brande does not enter into any detailed analysis of it. He however relates some experiments made to satisfy himself, that when serum has been coagulated by heat, after the addition of an acid, or by voltaic electricity, the serosity that remains contains no gelatine. He examines also what quantity of iron might be present, by evaporating a pint of serum to dryness, and then incinerating the residuum. When the ash thus obtained had been dissolved in nitro-muriatic acid, a copious precipitation of phosphate of lime took place on the addition of ammonia, but only a slight trace of oxide of iron.

By similar examination of the crassamentum of blood, the quantity of iron contained in it was also found to be extremely small, and not perceptibly different, by previously washing the crassamentum, so as to free it from its red particles. And in conformity to this experiment, when a quantity of colouring matter had been allowed to subside from serum, through which it was diffused, and then examined separately by evaporation, incineration, and re-agents, as before, the traces of iron, even in the red part, were found to be as indistinct as in the rest of the blood.

In order to procure colouring matter for experiment, Mr. Brande generally employed venous blood, from which the fibrine was separated by stirring during its coagulation, and the red globules were then allowed to subside from the serum, through which they thus remain diffused.

The effect of water upon these globules is to dissolve their colouring matter, and leave them colourless. If the solution be heated to near 200°, the colouring matter is rendered insoluble, and falls to the bottom of a brown colour. It is also coagulated by alcohol or by sulphuric acid.

Muriatic acid, poured upon the colouring matter, renders a portion of it insoluble, but dissolves a part, forming a solution, which appears crimson by reflected light, but green by transmitted light.

The colour of this solution is turned brown-red by supersaturation with caustic potash, but rather improved by soda or by ammonia. A portion of the muriatic solution, being evaporated in a water-bath,

retained its red colour to the last; but when quite dry, it became of a dirty red colour.

Sulphuric acid, diluted with eight or ten parts of water, being poured upon the colouring matter, if no heat be applied, remains perfectly colourless; but, by the assistance of heat, it forms a lilac solution, which remains unaltered for a great length of time, though exposed to light; but if heat be applied, so as to evaporate part of the water, the colour is destroyed in proportion as the acid becomes more concentrated.

The effect of nitric acid is to destroy the colour in greater or less time, in proportion to the quantity employed.

Acetic acid dissolves the colouring matter, with appearances similar to those of the muriatic solution.

The solution in oxalic acid is of a brighter red than any other hitherto noticed. In tartaric acid the solution approached to scarlet.

The alkalies also, or their subcarbonates, dissolve the colouring matter; and the solutions may be evaporated nearly to dryness without losing their red colour.

The next object of Mr. Brande was to find such combinations of the colouring matter as would be insoluble, and might therefore afford a permanent dye. When combined with alumina it is red while moist, but becomes brown when dried. With oxide of tin it may also be combined, but becomes of a dull red by drying; neither does supertartrate of potash give permanent brilliancy to the colour. But when a piece of calico has been previously dipped into infusion of oak-bark, and afterwards steeped in an alkaline solution of the colouring matter, it acquires a redness nearly equal to that given by madder, and tolerably permanent. But the most effectual mordants appeared to be some of the solutions of quicksilver. Pieces of woollen cloth, calico, or linen, steeped first in a solution of corrosive sublimate, and afterwards in a solution of the colouring matter, acquired a permanent red tinge, which remained unaltered by washing with soap.

The author has, therefore, considerable hopes that this substance may be of some utility in the art of dyeing; and he remarks, that blood has, in fact, been already employed by the Armenian dyers, along with madder, to ensure the permanency of the colour.

Observations of a Comet, with Remarks on the Construction of its different Parts. By William Herschel, *LL.D. F.R.S.* Read December 19, 1811. [*Phil. Trans.* 1812, *p.* 115.]

The author first gives us, in detail, the succession of appearances that he has observed respecting this comet and its various parts, consisting of a planetary body, perceptible only by the best telescopes, in the luminous spherical head, which to the naked eye appears as a nucleus. The head is surrounded by an envelope that is hemispherical on the side towards the sun, but extends in an opposite direc-

tion in the form of a very long cone of light, called the tail of the comet.

The planetary body was at no time perceptibly otherwise than circular. Its apparent magnitude was about three quarters of a second, and its real diameter is estimated at 428 miles. The position of this body never appeared to be in the centre of the head, but to be more or less eccentric at different periods of observation, but always more remote than the centre from the sun.

Nevertheless, the greatest illumination of the surrounding head is represented by Dr. Herschel as greatest in the centre, and in its decrease from thence to be somewhat brighter on the side toward the sun than at the part occupied by the planetary body. The apparent magnitude of the head was found to measure 3¾ minutes; so that its real magnitude is estimated to have been 127,000 miles.

Between the head and the surrounding envelope there was a space comparatively dark, which Dr. Herschel imagines to be filled with an elastic atmosphere, and estimates its actual extent to be at least 507,000 miles, since its apparent diameter was nearly 15 minutes of a degree.

The train of light, to which Dr. Herschel gives the name of envelope, from its surrounding the head on one side in a semicircular form, was found to measure 19 minutes of a degree in diameter, and was thence inferred to be 643,000 miles in real extent.

The two extremities of this curve being continued beyond the head in two streams of light, rather divergent from each other, form the appearance which is called the tail. The distance to which this appears to reach from the head varied on different nights, according to the state of the atmosphere, as well as from other circumstances which affected its actual length. The greatest length observed by Dr. Herschel was on the 6th of October, when he measured it 25°; but he thinks the measure of 23½°, taken on the 15th of October, more to be depended upon; and he thence computes the actual length to have been at that time 100,000,000 of miles.

With respect to the curvature of the tail, Dr. Herschel remarks, that it varied not only in degree, but in direction; for on the 2nd of December he observed that it appeared convex on the following side, as if the extremity of the tail preceded the head instead of being left behind.

The author also notices many other irregular appearances of the tail, the branches on each side occasionally dividing into two or three parts, and sometimes one branch, sometimes the other, seemed longest.

From the appearances observed, Dr. Herschel next infers what is the real construction of the various parts. And, first, the planetary body seems to be spherical, as might be expected from the common laws of gravitation, and to shine by light of its own; for if it were not so, it must have appeared to change its figure in moving as it did through more than a quadrant while it remained visible. The head also must, for the same reason, be spherical: and so likewise that portion of the envelope which is on the side towards the sun

must be hemispherical; for if it were merely a band of light, all in the same plane, its phases must have varied like the ring of Saturn. This cap must also be comparatively thin, since the parts at a distance from its edge, which were therefore seen transversely, appeared dark in comparison to the circumference, where a greater quantity of luminous matter was seen by oblique vision. And it is to the same cause that the comparative brightness of the edges of the tail is ascribed by Dr. Herschel.

With respect to the production of some of the cometic phenomena, the author conjectures, that the light is of a phosphoric nature; that the luminous matter of the head, being expanded on one side by the action of the sun, occupies more space, and consequently occasions the planetary body to appear eccentric; that part of this matter, being greatly rarified, ascends in the cometic atmosphere till it occupies the surface of that medium on the side towards the sun, and forms the hemispherical part of the envelope. He next supposes a further attenuation and a decomposition of this matter, till its particles are sufficiently minute to receive a slow motion from the impulse of the solar beams, and consequently *gradually* to recede in a direction towards the region of the fixed stars, to the distance of 100,000,000 miles.

From the escape of such a quantity of light, and probably of other subtile elastic matters, in consequence of the comet's near approach to the sun, Dr. Herschel infers that a greater consolidation of the remaining solid matter of the comet takes place at the time of its perihelion passage. He further thinks it not unlikely that the matter they contain is derived from nebulæ, which they meet with in the extensive orbits they describe; that in their course they visit other suns beside our own; and at each successive approach to these various centres they undergo progressive condensation; from which we may conceive how other planetary bodies may begin to have existence.

On a gaseous Compound of carbonic Oxide and Chlorine. By John Davy, *Esq. Communicated by Sir* Humphry Davy, *Knt. LL.D. Sec. R.S.* Read February 6, 1812. [*Phil. Trans.* 1812, *p.* 144.]

Although it has been asserted by Messrs. Gay-Lussac and Thenard, and also by Mr. Murray, that carbonic acid and chlorine have no action upon each other, Mr. J. Davy has observed the contrary to be the case. A mixture of equal parts of these gases, previously dried over mercury, being exposed to bright sunshine for about one quarter of an hour, lost all colour of the chloric gas, and were found condensed into half their former volume. The smell of this gas was more suffocating than that of chlorine. It occasioned a very painful sensation in the eyes; it reddened litmus paper; it combined with ammonia, forming a salt perfectly neutral and dry, but deliquescent by attracting moisture from the atmosphere. This salt was decomposed by sulphuric, nitric, and phosphoric acids, and also by liquid muriatic

acid; but sublimed unaltered in carbonic, sulphureous, and muriatic acid gases.

In those instances where the salt was decomposed, the products were carbonic and muriatic acid gases. It is remarkable, that in the formation of this ammoniacal salt, the new gas combines with as much as four times its bulk of ammoniacal gas.

Tin, zinc, antimony, or arsenic, heated in this gas, combine with the chlorine, forming the usual compounds, and extricate carbonic oxide. The decomposition thus effected is completed in a few minutes, but without explosion or ignition. Nor was even the action of potassium violent, although the decomposition was more complete; since the carbonic oxide itself was decomposed, and carbon deposited. When the white oxide of zinc was employed instead of the metal, the gas that remained was found to be carbonic acid.

Sulphur or phosphorus might be sublimed in this gas without effecting any change upon it. Neither did hydrogen or oxygen singly produce any effect by means of the electric spark; but when both were mixed in due proportion of two parts hydrogen with one oxygen to two of the gas, an explosion took place, with the formation of muriatic acid and carbonic acid.

The power of this gas as an acid is such as to decompound dry subcarbonate of ammonia; and its power of saturation is so remarkable, that the quantity of ammonia, which was not saturated by two measures of carbonic acid, became fully saturated by only one measure of the new gas, which had extricated those two measures.

For the formation of this gas, it is absolutely necessary that its constituents should be in a state of perfect dryness, otherwise it will be mixed with carbonic acid and muriatic acid, in proportion to the quantity of moisture present; for if water be admitted to it, the whole is decomposed.

If they be mixed in a receiver previously exhausted, the direct light of the sun is not necessary; daylight alone is sufficient to unite them in less than twelve hours. But when they are mixed over mercury, then it is requisite to expose them immediately to bright sunshine; for otherwise the chlorine is absorbed by the mercury, and carbonic oxide alone remains.

The author also has endeavoured to unite these gases in an earthenware tube heated to redness, but without success.

The specific gravity of the gas has not been directly measured, but is inferred from the diminished bulk which it has been observed to occupy, and the known weights of the two gases which compose it. From these data, 100 cubic inches of gas are estimated to weigh 105·97 grains.

The relative affinities of chlorine for carbonic oxide or for hydrogen, and of carbonic oxide for chlorine or for oxygen, appeared to the author to be very nicely poised.

Hydrogen does not decompose the new gas, neither does carbonic oxide decompose muriatic gas; and further, if equal parts of chlorine, of hydrogen, and of carbonic oxide, be mixed and exposed to light,

the chlorine is found to divide itself equally between the other two.

So also with respect to the relative affinities of carbonic oxide for chlorine or for oxygen. The new gas is not decomposed by oxygen, neither is carbonic oxide altered by admixture with any proportion of chlorine that has been tried.

A Narrative of the Eruption of a Volcano in the Sea off the Island of St. Michael. By S. Tillard, *Esq. Captain in the Royal Navy. Communicated by the Right Hon. Sir* Joseph Banks, *Bart. K.B. P.R.S.* Read February 6, 1812. [*Phil. Trans.* 1812, *p.* 152.]

The eruption here described by Capt. Tillard was first observed by him on the 12th of June, 1811, having burst forth only two days before. It had been preceded by another eruption in the month of January, about three miles distant. Having come to anchor on the 13th in the road of Porta del Garda, Capt. Tillard set off on the following morning with some other gentlemen, for the purpose of witnessing the phenomena from the adjacent cliffs of St. Michael. The place of the eruption was scarcely a mile from the base of the cliff, which was nearly perpendicular, and about 400 feet high.

In the most quiescent state of the volcano, there appeared a circular body of smoke over the surface of the water, in continual rotatory motion, extending itself to leeward; but suddenly a column of very black ashes and cinders would shoot up, in the form of a spire, inclined from ten to twenty degrees from the perpendicular; and, again, a second, third, and fourth column, each overtopping the preceding, till the last appeared more above the level of the eye than the sea was below it.

When the first impetus that raised the column ceased, the smoke was seen to break into various fanciful forms; some ascending by their proper levity, others carried downwards by the particles of falling ashes, so as to give the appearance of pendent plumes of black and white.

These bursts were always accompanied by vivid flashes of lightning in the densest part, and followed by a succession of waterspouts that appeared drawn up by the masses of smoke as they rolled away before the wind.

The part of the sea where the volcano was situated was known to be full thirty fathoms deep; but in the course of the time that Capt. Tillard and his friends were watching it, a ridge was seen to project above the surface of the water; and before they quitted the cliff, which was in about three hours, a complete crater was formed, apparently 400 or 500 feet in width, and elevated on the leeward side not less than twenty feet in height.

The great eruptions were generally attended with sounds like the firing of cannon or musquetry, and often with slight shocks of earthquake.

On the next day the volcano continued to emit clouds of black smoke and ashes, but was comparatively tranquil.

On the succeeding day, however, the eruptions were repeated with still greater violence than before; and the quantity of matter thrown up subsequent to this period was so great, that upon Capt. Tillard's return to St. Michael's on the 4th of July, one side of the crater was elevated nearly eighty yards above the level of the sea, and the circuit of it so nearly complete, that the channel of communication between the inside and outside was not more than six yards over, and the water within was boiling hot. The beach was also proportionally heated; so that although by rowing round to the leeward side Capt. Tillard was able to land on the outer margin, the heat prevented his ascending at that part more than a few yards. The inclination also was so steep on all sides, as to occasion considerable difficulty in the attempt to reach the summit. The declivity below the surface of the sea was such, that at the distance of twenty or thirty yards the depth was found to be twenty-five fathoms.

A portion, about sixty feet in length, on one side of the opening being separated into a sort of peninsula, this part was chosen for ascending, by means of a narrow isthmus of cinders, that connected it with the rest of the circumference of the crater.

When Capt. Tillard had ascended the ridge, it was found too narrow to walk upon, the descent within being as steep as that on the outside. But the ridge gradually widened toward the other extremity, which was elevated between twenty and thirty feet from the sea, with a flat top, bounded by a precipice on one side of the channel of entrance.

Within the crater was found the skeleton of a guard-fish, so burned as to break to pieces on attempting to take it up; and it was said that great numbers of fish had been destroyed by the eruption, and thrown dead upon the coast of St. Michael.

The general material of which this mound consisted, was found to be a spongy substance like cinders, to which stones had been reduced by the action of heat; but there were also other portions of stone that had undergone no such alteration.

On the primitive Crystals of Carbonate of Lime, Bitter-Spar, and Iron-Spar. By William Hyde Wollaston, *M.D. Sec. R.S.* Read February 13, 1812. [*Phil. Trans.* 1812, *p.* 159.]

In consequence of the supposed agreement of these three minerals, in the same primitive form of their crystals, the two latter have been arranged by the Abbé Haüy among those varieties of carbonate of lime which contain substances foreign to its proper chemical nature.

It has been objected to M. Haüy, that the magnesian carbonate of lime, or bitter-spar, is a proper chemical compound, and as such should have a form different from that of mere carbonate of lime; and that since iron-spar frequently contains little or no lime, its crystalline form should also be different.

It is now found by the author of the present communication, that such differences as the theory appeared to require do actually exist.

Respecting the primitive rhomboid of carbonate of lime, he has already communicated to the Society an observation, that its angle is greater by full half a degree than that assigned to it by crystallographers; and he now adds two corresponding observations respecting those substances which are so nearly allied to it.

By employment of the same improved method of measurement by means of the reflective goniometer, he has found that the obtuse angle of the primitive rhomboid of bitter-spar, exceeds that of carbonate of lime by full 1° 10′; and that the corresponding angle of iron-spar exceeds the same angle by nearly 2°, and accordingly is, in fact, 2½° greater than former measures had given it.

The angle of carbonate of lime is here said to be 105°, and nearly 5′. That of bitter-spar 106¼°; that of iron-spar 107°. And since in the last instance the author found the substance under examination to be wholly free from lime, he infers that when the same form occurs in other specimens that do contain carbonate of lime, it does not depend on the presence of that ingredient, but depends on the carbonate of lime alone.

He thinks it, however, possible, that in certain mixtures each of these substances may exert their crystalline powers; and in consequence of the near agreement of their primitive angle, may occasion that degree of curvature of the surfaces which gives the peculiar lustre of what is called pearl-spar.

Among the varieties of these minerals which contain manganese, the author has thought it not improbable that the form of some of them might be altered or modified by its presence; but he has not hitherto succeeded in detecting any other form which could be ascribed to that ingredient.

Observations intended to show that the progressive Motion of Snakes is partly performed by means of the Ribs. By Everard Home, *Esq. F.R.S.* Read February 27, 1812. [*Phil. Trans.* 1812, *p.* 163.]

In the cobra di capello, Mr. Home formerly observed to the Society, that the power which it possesses of elevating its hood, depends on the motion of the ribs of the neck, which have a peculiar form adapted to that purpose. He has lately found that this motion is not, as he then supposed, confined to those ribs alone of that snake, but appears to be common to all the ribs of the whole tribe of snakes.

Mr. Home acknowledges himself indebted to the President, who first remarked an apparent motion of the ribs in succession, like the feet of a caterpillar, in a large coluber, brought for his inspection into his library. And Mr. Home, by placing his hand underneath the belly of the snake, distinctly felt the ends of the ribs press in succession on the palm of the hand as the animal passed over it.

By examining the skeleton of a large boa, formerly sent from India by Sir William Jones, and now deposited in the Hunterian collection, the structure of the ribs which adapts them for such motion was very evident, and is described by the author with figures, which show a

distinct attachment of a rib on both sides to each vertebra by a ball and socket-joint. It is remarked, that in this tribe of animals the relative positions of the ball and socket are reversed from their usual situation, the socket being attached to the extremity of the rib, and fitted to a protuberance from the body of the vertebra, instead of the extremity of the rib being applied to an indentation between two adjacent vertebræ. Hence the ribs do not in any degree interfere with the motion of the vertebræ upon each other, as in other animals.

The muscles by which these motions are performed, are also described by Mr. Home; but the distribution of them cannot readily be understood, without reference to the drawings which accompany the paper.

At the termination of each rib is a small cartilage, which rests for its whole length on the inner surface of the corresponding abdominal scutum, to which it is connected by a short muscle.

The scutum being thus moved by a rib from each side, its posterior edge lays hold of the ground, and becomes the support by which the adjacent portion of the body is propelled forwards, and by a series of alternate motions is capable of renewing the impulse with considerable rapidity.

Mr. Home remarks, that in the *Draco volans* the wings, by which the animal flies, are supported by ribs, which form the skeleton of them; but in this instance the elongated ribs are superadded, for the sole purpose of forming the wings, and do not, as in the snake, assist in the process of respiration, at the same time that they are employed in giving progressive motion.

An Account of some Experiments on the Combinations of different Metals and Chlorine, &c. By John Davy, *Esq. Communicated by Sir* Humphry Davy, *Knt. LL.D. Sec. R.S.* Read February 27, 1812. [*Phil. Trans.* 1812, *p.* 169.]

The principal objects of these experiments is to determine the proportions in which oxymuriatic acid or chlorine combines with various metals; but the author has also extended his inquiry to the relative proportions, in which oxygen also, and sulphur, unite with some of the same metallic substances.

Of copper, Mr. John Davy notices two compounds, to which he gives the names of Cuprane and Cupranea. The former is the same as the resin copper of Boyle, which may be obtained by heating together one part of copper with two parts of corrosive sublimate. This compound is also the same as that named by Proust, white muriate of copper, who obtained it by mixing together muriates of tin and copper; and Proust observed that the same compound might be procured, by heat, from the common deliquescent muriate of copper.

This compound is fusible by heat below redness, and in close vessels is not decomposed by a strong red heat; but if air be freely admitted, it is dissipated in white fumes. It is insoluble in water, but

is soluble with effervescence in nitric acid, and without effervescence in muriatic. It consists of 36 chlorine, and 64 copper.

In the other compound, which the author calls Cupranea (a term that, in the language proposed by his brother, implies a substance containing more acid), the proportions are 53 chlorine, and 47 copper. This compound is also best obtained from the common deliquescent muriate by slow evaporation, carried ultimately to dryness, at a temperature not exceeding 400° of Fahrenheit; for if greater heat be applied, one portion of chlorine is expelled, and what remains is reduced to the state of cuprane and resin copper.

In addition to the foregoing, there is also a native muriate of copper, which, by the author's analysis, consists of 73 brown oxide, 16·2 muriatic acid, and 10·8 water.

This compound has also been imitated by Proust; and Mr. J. Davy has also found various methods of making the same combination.

Tin also forms two compounds with chlorine, one already known under the name of the fuming liquor of Libavius, which the author calls Stannanea, most readily obtained by heating together an amalgam of tin with corrosive sublimate; and a second analogous to the former, made by the substitution of calomel, and accordingly containing a less proportion of chlorine, and therefore called stannane.

The former contains 42·1 tin, with 57·9 chlorine.
The latter ——— 62·22 tin, — 37·78 chlorine.

The only new and remarkable property of the liquor of Libavius observed by Mr. J. Davy, is its action upon oil of turpentine, which in one experiment was so violent as to occasion inflammation. In other instances oxide of tin seemed to be formed, and a tenacious oil, having a smell somewhat like camphor.

Beside these compounds of tin with chlorine alone, there is also a submuriate observed by Proust, containing about 70·4 grey oxide, 19 muriatic acid, and 10·6 water.

With iron likewise, as well as with the former metals, there are two compounds with chlorine, which may either be formed by direct union with oxymuriatic gas, or may be obtained by evaporating to dryness the green and red muriates of iron. When thus deprived of water, they receive the name of Ferrane and Ferranea. The former contains 46·57 iron, and 53·43 chlorine; the latter 35·1 iron, and 64·9 chlorine.

With other metals that have been tried by the author, such as manganese, lead, zinc, arsenic, antimony, and bismuth, he has not found that chlorine combines in more than one proportion.

The compound with manganese bears a red heat, in close vessels, without decomposition; but when it is heated in an open vessel, muriatic acid fumes are evolved, and oxide of manganese remains. It appears to consist of 54 chlorine, and 46 manganese.

The muriate of lead, known by the name of Horn lead, but called by the author Plumbane, was found to contain 74·23 lead, and 25·77 chlorine.

Butter of zinc, obtained by distilling to dryness the muriate of zinc, was found to consist of exactly equal parts of the two ingredients.

The fuming butter of arsenic was found to contain nearly 60 chlorine to 40 arsenic. This compound has the property of dissolving phosphorus when gently heated, but to part with it on being cooled. It also readily dissolves sulphur when warmed, and yields crystals of sulphur by cooling. It likewise dissolves resins, oil of turpentine, or olive oil; and in these respects resembles the compounds of sulphur or phosphorus with chlorine, which have the same property of entering into combination with fixed and volatile oils.

In this respect also the butter of antimony was found to agree with the same compounds; and in the proportion of its constituent parts, to be as nearly as possible the reverse of the preceding.

It is remarked concerning these compounds, in general, of metals with chlorine, that their volatility or fusibility are in no degree correspondent to the qualities of the metals of which they consist. One of the compounds of iron, for instance, is volatile; but those of bismuth, zinc, and lead, are even less fusible than the metals themselves.

In order to correct the preceding analysis, the author has had recourse to the general analogy of definite proportions; and since one of hydrogen unites with 7·5 oxygen, or with 33·6 chlorine, the compounds of chlorine have been compared with the oxides of the same metals; and in the instances of copper, iron, zinc, and arsenic, have been found to agree correctly with the analyses of oxides by other chemists; and where such agreement has been wanting, it has been obtained by new analyses of the oxides of tin, lead, antimony, and bismuth.

In making similar comparisons of the compounds of the same metals with sulphur, four instances of correspondence were found in tin, lead, antimony, and bismuth; but others were observed not to accord with the proportions assigned.

The author concludes with observing the degree of analogy that subsists between the oxides of metals, and their compounds, with chlorine; horn silver, resin copper, horn lead, and corrosive sublimate, being each soluble in excess of muriatic acid, although the last is even less soluble in nitric or sulphuric acids than in mere water.

Further Experiments and Observations on the Action of Poisons on the Animal System. By B. C. Brodie, *Esq. F.R.S. Communicated to the Society for the Improvement of Animal Chemistry, and by them to the Royal Society.* Read February 27, 1812. [*Phil. Trans.* 1812, *p.* 205.]

In the description of the author's former experiments on the same subject, he entered more into the detail of particular occurrences than he now thinks necessary. He was formerly apprehensive that the operations of the same poison might not be always the same, and was

therefore careful to relate all the circumstances. But he now finds extremely little difference in the action of the same poison, even upon different animals; and in those of the same species no difference but what may be referred to difference of quantity of poison, or age, and power of the animal. He consequently does not enter minutely into the particulars of his late experiments, but gives a general account of those which appear to be of most importance, with regard to the inferences that may be drawn from them. The greatest part of them relate to the action of mineral poisons; but since, on the former occasion, his trials of woorara had been left imperfect for want of a sufficient quantity of that poison, he states the results of two experiments made after obtaining a fresh supply, for the purpose of endeavouring to recover animals that had been apparently killed by it.

A young cat was the subject of the first experiment. In four minutes after the application of woorara to a wound in her side, she appeared to be dead, but the heart continued to act 140 times in a minute. Mr. Brodie then inflated the lungs, and repeated the artificial respiration forty times in a minute. At the end of forty minutes the pupils of the eyes were observed to contract by an increase of light upon them; but in other respects she was motionless and insensible. At the end of an hour further symptoms of life began to appear, and there was an effort to breathe occasionally. There were also various involuntary motions. The efforts to breathe became gradually more frequent, and after two hours had elapsed, the spontaneous efforts were repeated as often as twenty-two times in a minute.

The artificial respiration being then discontinued, she lay as if in a profound sleep for about forty minutes, when she suddenly awoke, and gradually recovered from all the effects of the woorara.

A second experiment, of the same kind, performed on a rabbit, was not so successful; for though the action of the heart was continued strong and regular for more than three hours by means of artificial respiration, there never was the least appearance of returning sensibility; and the pulse from that time began to subside, and ultimately ceased altogether.

The mineral poisons here examined by Mr. Brodie, are arsenic, muriate of barytes, emetic tartar, and corrosive sublimate.

When arsenic is taken internally, it is observed that some appearance of inflammation of the stomach is usually found after death; and the general opinion is, that this inflammation is caused by the local application of the arsenic to the coats of the stomach; and secondly, that this inflammation is the cause of death.

To these opinions Mr. Brodie objects, that in many cases the appearances of inflammation are too slight to warrant such an opinion; and in most instances of animals killed by arsenic, death takes place in too short time for it to be considered as the result of inflammation.

The author observes also, that the inflammation does not depend on the local application; for it has been remarked by Mr. Hunter and Mr. Hume, and Mr. Brodie has confirmed the observation by new experiments, which have satisfied him, that inflammation of the

stomach is more violent and more immediate, in consequence of application of arsenic to wounds, than when it is taken into the stomach itself. The symptoms first produced are paralysis of the hind legs, and other parts of the body; convulsions, dilatation of the pupils, and general insensibility, indicating disturbance of the functions of the brain. Secondly. A feeble slow intermitting pulse, from disturbance of the functions of the heart. Thirdly. Pain in the abdomen, sickness, vomiting, &c. from the action of this poison on the stomach and intestines.

From such general affection of such different organs, from the order in which they are affected, and from the analogy with vegetable poisons, which from Mr. Brodie's former experiments appeared not to act till they had entered the circulation, he infers that arsenic also, in whatever way administered, does not produce its effects even on the stomach till it has been received into the blood. And although it might be supposed that blood containing arsenic would equally destroy the vitality of every part, there are various circumstances which show that this is not the case; for even to the instant of death, in consequence of the full effect of arsenic on the brain, heart, and bowels, various secretions continue apparently unimpaired; and even after death the muscles remain excitable to powerful contractions by means of electricity.

From such trials as Mr. Brodie has made of muriate of barytes, it has appeared to act principally on the brain, but in some degree on the heart also; for although the heart always continued to act after respiration had ceased, in consequence of the affection of the brain, nevertheless the pulse was feeble and intermittent; and although artificial respiration was made with the greatest care, in the hope of keeping up the circulation, it could rarely be maintained more than a few minutes. In some instances, in which the artificial respiration had been pursued with apparent success for a greater length of time, there were some signs of restoration of the functions of the brain, with occasional returns of voluntary respiration; but though the greatest care had been taken to preserve the temperature also by external means, the pulse notwithstanding continued to diminish in strength and frequency, and ultimately ceased. It was found, however, that the heart, the intestines, and the muscles, in general, were excitable to contraction by means of electricity, although insensible to the stimulus of blood poisoned by muriate of barytes.

The stomach likewise is affected by this poison with some degree of inflammation, but less so than by arsenic. And though in some instances it operates as an emetic, the author considers the inflammation to be independent of the local application, and to arise through the medium of the general circulation.

The effects produced by emetic tartar are very similar to those of the preceding poisons; but it much more frequently occasions vomiting. The symptoms enumerated are paralysis, drowsiness, and at last complete insensibility. The pulse becomes feeble, but still the heart continues to act after apparent death; so that its action

may be increased by artificial respiration, but never longer than for a few minutes. Its principal action is conceived to be upon the brain; but it appears to act upon the heart also. The stomach sometimes bears marks of inflammation; but the author has seen no instance of the intestines being inflamed.

The experiments of Mr. Brodie on corrosive sublimate, have led him to conclusions very different from the preceding respecting its mode of action. When applied to a wounded part, it produces a slough, without any affection of the general system; and when taken internally, its effects, he thinks, may be best explained by its local action on the stomach alone, unconnected with any absorption of it into the circulation.

When a solution of corrosive sublimate is taken into the stomach, the mucous membrane is found of a dull grey colour, having lost its texture, so as to be easily separated from the muscular coat; and as this precludes the idea of absorption into the circulation, Mr. Brodie conceives that its deleterious effects depend entirely upon its chemical action on the stomach, and that the brain and heart are thence affected by nervous sympathy.

Observations of a second Comet, with Remarks on its Construction. By William Herschel, *LL.D. F.R.S.* Read March 12, 1812. [*Phil. Trans.* 1812, *p.* 229.]

The tendency of Dr. Herschel's observations is to point out a difference between this second comet and the former, of which he lately gave an account to the Society.

The latter appeared to him as a nucleus about 5″ in diameter, surrounded by a very faint chevelure. Since this appearance on the two first days of observation was not well defined, Dr. Herschel was in doubt whether to consider the nucleus as corresponding to the head of the former comet, or to the bright planetary body that he had observed in its centre; being extremely small in comparison to the head, and as much too large to be supposed of a planetary construction. But on two subsequent days of observation, the nucleus was pretty well defined, even with a power of 170; and the author was led to consider the latter as the more probable opinion. On the fifth day of observation (which was the last time that it could be seen, by reason of the interference of the moon's light), Dr. Herschel attended carefully to the magnitudes of the body, as it appeared to different magnifiers; and by subsequent comparison on the following morning of objects of known diameter with his recollection of these magnitudes, he determined the measure of the nucleus to have been 5″·2744.

Since the distance of this comet from us was at that time rather greater than that of the sun, the real magnitude of the diameter thus measured is estimated at 2637 miles.

As the light of the chevelure was too feeble to be seen at this time, on account of the light of the moon, its greatest extent, in a direc-

tion opposite to that of the sun, was estimated from the observations made two days preceding, when it measured about 9′ 40″, and its length, consequently, 659,000 miles.

Dr. Herschel remarks, that the physical construction of this comet must have been extremely different from that of the former, approaching very nearly to the planetary condition, and having a diameter nearly one third that of the earth.

The light by which it was seen he also considers as planetary: that is to say, reflected from the sun, and not phosphorescent, like the preceding; for if this were self-luminous, says Dr. Herschel, we could hardly account for its little density, which would scarcely bear to be magnified even to 107 times, although the former was seen with a power of 600 even better than with one that was lower.

The chevelure, however, he conceives to consist of phosphoric matter suspended in an elastic atmosphere that surrounds the body of the comet; and he ascribes the faint appearance of this chevelure, according to the theory advanced in his late communication, to the existence of a very small quantity of nebulous matter, which had not been consolidated by passing through a perihelion. And hence, says the author, this last comet was but little more affected by a perihelion passage than a planet would have been.

Additional Experiments on the Muriatic and Oxymuriatic Acids. By William Henry, *M.D. F.R.S. V.P. of the Lit. and Phil. Society, and Physician to the Infirmary, at Manchester.* Read March 19, 1812. [*Phil. Trans.* 1812, *p.* 238.]

In consequence of the discussion which has lately taken place concerning the nature of these acids, the author has been induced to repeat, with more perfect apparatus than he formerly possessed, a part of those experiments of which he published an account in the Philosophical Transactions for 1800, and to add others tending to elucidate the same subject. Those experiments in general related to the *electrization* of muriatic acid gas; but there was also one experiment in which he endeavoured and supposed that he had succeeded in extracting water from it, by means of muriate of lime, as sensible heat was evolved as soon as the muriate of lime was brought into contact with the gas. But he has since found that the evolution of heat occurs only when the muriate of lime has attracted moisture either from the atmosphere or from the mercury through which it is passed; for then it condenses a portion of the acid gas.

In his present experiments on electrization, Dr. Henry confirms his former results with regard to the evolution of hydrogen by that means; and he observes, that when the electrization of muriatic acid gas is performed over mercury, the hydrogen evolved amounts to about one fifteenth of the original quantity of gas employed. There appears, however, to be a contraction of volume, in consequence of the absorption of a part of the acid to form calomel. When the hydrogen amounts

to the quantity which has been stated, nothing further is gained by continuation of the process.

When mercury is not present during the electrization, then a different result is obtained. The quantity of hydrogen is estimated by the author not to exceed one seventieth part of the gas employed. The gas does not appear to be changed in bulk; but a part of it is converted into oxymuriatic acid gas. When this gas amounts to a certain proportion of the mixture, the effect of the electricity will then be to re-unite it to the hydrogen, and form muriatic acid gas rather than to decompose it.

But when mercury is present, it removes the oxymuriatic acid as fast as it is formed; and then a larger proportion of hydrogen is evolved, till every nascent portion of oxymuriatic acid gas is so surrounded by hydrogen that it cannot attain to a state of actual extrication.

In support of this explanation, Dr. Henry mixed thirty measures of hydrogen with 400 of muriatic acid gas; and when this mixture was electrified over mercury, no effect was produced; the quantity of hydrogen was not increased; and it was evident that no oxymuriatic gas was evolved, as the surface of the mercury remained without the slightest tarnish.

When, on the contrary, the muriatic acid gas was mixed with oxygen, and electrified over mercury, then a diminution of bulk ensued, and the surface of the mercury became tarnished, as by the contact of oxymuriatic gas; and water was formed in drops, and deposited on the inner surface of the vessel, combined with a portion of the muriatic acid.

These results, says Dr. Henry, may be explained either according to the commonly received theory, or according to that adopted by Mr. Davy.

According to one view the oxygen unites to the real acid of the muriatic gas, and forms oxymuriatic gas, which then deposits water that had been previously held in solution. According to the other, the oxygen unites with the hydrogen, and forms water, while the oxymuriatic base is now disengaged as a simple body.

The author, at present, is not acquainted with any ground for giving a preference to one above the other of these two modes of explanation; but he imagines that something would probably be gained by a precise determination of the proportions in which the gases saturate each other. He has not, however, been able to satisfy himself on this point; and he conceives that the condensation of a portion of the muriatic acid gas by the water that is formed during the process, is an almost insuperable impediment to any precise determination of these proportions.

Of the Attraction of such Solids as are terminated by Planes; and of Solids of greatest Attraction. By Thomas Knight, *Esq. Communicated by Sir* Humphry Davy, *LL.D. Sec. R.S.* Read March 19, 1812. [*Phil. Trans.* 1812, *p.* 247.]

The attention of most mathematicians who have treated of the attractions of bodies, has been confined to those bounded by continuous surfaces; and Mr. Knight is not aware that any author, with the exception of Mr. Playfair, has given an example of that kind of inquiry which he here undertakes.

If a solid be bounded by plane surfaces on all sides, whether regular or irregular, he undertakes to determine its action, both in quantity and direction, upon any point placed either within or without the body.

For this purpose, the solid is first conceived to be divided into its most simple forms, of which the action can be determined separately; and thence the collective force of the aggregate is subsequently ascertained.

The first section treats of the attraction of planes bounded by right lines (whether triangular, quadrangular, or polygonal), on points however situated.

The second section extends the same inquiry, first to pyramids, and then to solids, which may be divided into as many pyramids as there are sides.

And in the third, the attraction of prisms of various forms is investigated.

Having completed this part of the subject, Mr. Knight next applies the formulas he has obtained to find the attraction of certain complex bodies, which, though not bounded by planes, have a natural connexion with the preceding subject, having their sections in one direction of a right-lined figure, though in another direction their sections be in part curvilinear, such as the portion of a cylinder generated by the motion of a segment of a circle parallel to itself, or parabolic cylinder by similar motion of a portion of a parabola.

Under the same head, also, the author includes curvilinear pyramids, or groined solids cut from the preceding cylinders by a transverse motion of a similar or dissimilar curve, so as to have a parallelogram for their base.

In the section which concludes this communication, the author enters into the consideration of solids of greatest attraction; for though this subject has been already treated of, in part, by Professor Playfair and by Silvabelle, their investigations relate solely to homogeneous solids of revolution; but Mr. Knight extends the investigation to the attractions of those solids treated of in the preceding sections, not only when the density is homogeneous, but also according to different hypotheses of varying density.

Of the Penetration of a Hemisphere by an indefinite Number of equal and similar Cylinders. By Thomas Knight, *Esq. Communicated by Sir* Humphry Davy, *LL.D. Sec. R.S.* Read March 19, 1812. [*Phil. Trans.* 1812, *p.* 310.]

The problem which Mr. Knight here undertakes to solve, is to pierce a hemisphere perpendicularly to the plane of its base, with any number of equal and similar cylinders of such kind, that after the removal of these cylinders, the remainder of the hemisphere shall admit of exact cubature; and when the surface has been thus perforated, the remaining surface shall admit of exact quadrature.

But the solution of this problem, as well as of those contained in the preceding communication, was, of course, such as not to admit of being publicly read.

On the Motions of the Tendrils of Plants. By Thomas Andrew Knight, *Esq. F.R.S. In a Letter to the Right Hon. Sir* Joseph Banks, *Bart. K.B. P.R.S.* Read May 4, 1812. [*Phil. Trans.* 1812, *p.* 314.]

Some naturalists have supposed tendrils to be endued with some degree of perception, to which their propensity to approach neighbouring objects may be ascribed; and though others who have written on the same subject may have rejected this hypothesis, it does not appear to Mr. Knight that any direct experiments have been made similar to those which he here describes, for the purpose of ascertaining whether these motions may not be ascribed to peculiarity of organization, aided by the operation of external causes.

The plants selected for his experiments were, the Virginia Creeper, the Ivy, the Vine, and the Pea. When a young plant of the creeper, trained directly upwards, was placed alone in the centre of a forcing-house, its tendrils were all turned towards the north wall; but as this was out of their reach, they declined gradually, and ultimately fixed themselves on the upright stem beneath and upon its support.

When other plants were placed near the glass, their tendrils were always directed from the light, as in the former instance, although no object was within their reach on the dark side.

Mr. Knight next tried the effect of placing near them a piece of dark paper; to this they appeared to be strongly attracted, and when the paper was removed to a new position, the tendrils were found to follow it. When a piece of glass was substituted for the paper, the tendrils showed no disposition to approach it; but, on the contrary, when it was placed so as to reflect the light of the sun upon the tendrils, they appeared to be strongly repelled by it.

In making corresponding experiments on the ivy, Mr. Knight found the same propensity in its claspers to recede from the light; but it was necessary to place substances much nearer than in the former experiments for them to manifest any appearance of spontaneous motion.

When a young plant of the vine was placed under the same cir-

cumstances as the creeper, its tendrils manifested the same tendency to recede from the light. When first emitted, they pointed upwards; but the angle gradually increased, and ultimately they pointed directly downwards when no object was within their reach to which they could attach themselves. The ultimate direction was in all instances towards the darkness of the ground, whether the plants were trained upwards, horizontally, or downwards.

There was, however, this difference observable between the tendrils of the vine and those of the Virginia creeper: those of the vine could be made to return to any position which they had quitted, by changing the direction of the light; but those of the creeper never returned to a situation from which they had once receded; as the tendrils of the vine are, moreover, separated into two divisions, they do not often fail of coming into contact with adjacent objects; and the effect of contact is almost immediately visible. They bend more firmly toward the body, and attach themselves by twining round it.

The organization of the tendril, by which it is adapted to perform these motions, appears to the author very similar to that of the young succulent shoot. It is abundantly provided with vessels; and it seems not improbable that a very considerable quantity of the moving fluid of the plant passes through them, and that there is a close connexion between their vascular structure and their motion, as appears more especially in the act of grasping an object. The external pressure of the body on one side will probably impede the motion of the fluids on that side of the tendril, and occasion greater extension of the opposite side in giving passage to a greater proportion of sap. In conformity to this explanation, it is observed, that the sides of the tendrils that are in contact with the substance embraced are visibly compressed and flattened.

Observations on the Measurement of three Degrees of the Meridian conducted in England by Lieut.-Col. William Mudge. *By Don* Joseph Rodriguez. *Communicated by* Joseph de Mendoza Rios, *Esq. F.R.S.* Read June 4, 1812. [*Phil. Trans.* 1812, *p.* 321.]

After tracing the history of the several measurements that have been made from time to time in different parts of the world, the author observes, that little doubt would have remained as to the earth being flattened at the poles, had not the English measurement given an opposite result; the degree at the northern extremity being found equal to 60,766 fathoms, while that at the southern appeared to be 60,884.

Colonel Mudge's estimate of the linear measure of a degree is made by dividing the number of fathoms measured in linear extent of an arc by the number of degrees and parts of a degree ascertained by observations of stars. Don Joseph Rodriguez has followed a different course. He assumes as data the linear extent of Col. Mudge's base line, and the horizontal angles of his triangles ascertained by observation. He assumes, also, that the figure and dimensions of the earth

are known by other measurements, and thence infers what ought to have been the angles ascertained by Col. Mudge's astronomical observations.

Col. Mudge's base in fathoms is first converted into toises, of which the logarithm is taken as the logarithm of an arc. The radius of that arc being assumed from the supposed figure of the earth, its angular extent, its logarithmic sine, and chord, are inferred by the method of Delambre. The horizontal angles measured are then corrected for spherical error, so as to convert the triangles into spherical triangles, the sides of which are found first as logarithmic sines, and thence the logarithmic arcs are deduced. The azimuths of these being also known, the portions of the meridian to which they each correspond are computed both in angular and linear measure: the results are given by the author in two tables, one of which is deduced from the eastern series of triangles, and the other from the western series.

The angular extent of the whole arc thus calculated is 2° 50′ 21″·97. The observed angle differs from this by 1″·38, being 2° 50′ 23″·35.

In making separate estimates of the two portions of this arc, that are to the north and south of the central station at Arbury Hill, he finds that the former exceeds its calculated amount by 4″·77; while in the latter the calculated extent is greater than that deduced from astronomical observations by 3″·39. He therefore infers that the astronomical observations at Arbury Hill must be erroneous nearly to the amount of 5″, notwithstanding the goodness of the instruments and the skill and care of the observer.

By a mode of calculation similar to the foregoing, made upon the measurement in Lapland, Don Joseph Rodriguez arrives at a result which differs only 0″·6 from that of M. Svanberg; and by the same method, with respect to the measurement in Bengal by Major Lambton, the difference between observation and calculation is only 0″·53.

A portion of the French measurement between Dunkirk and Paris, similarly estimated, is not found to accord so well with the author's calculations, which make a difference of 2″·60 more than appeared by observation.

A similar disagreement, it is observed, was also found by M. Mechain in the very short distance between Montjui and Barcelona, the latitudes of which, as determined by a very long series of zenith distances, do not agree with the results of measurement by as much as 3″·24.

Local attractions were supposed to be the cause of this irregularity; and Col. Mudge also was of opinion, that the irregularities which occur in his measurements are to be ascribed to deviations of the plumb-line from local attractions. But the author of the present communication thinks they should rather be ascribed to the observations themselves than to any extraneous source; since the observations of different stars give results that differ more than 4″ from each other.

An Account of some Experiments on different Combinations of Fluoric Acid. By John Davy, *Esq. Communicated by Sir* Humphry Davy, *Knt. LL.D. Sec. R.S.* Read June 11, 1812. [*Phil. Trans.* 1812, *p.* 352.]

The subject of this communication is divided into four sections, the first of which relates to silicated fluoric acid gas, and to subsilicated fluoric acid; the second to the combinations of these acids, and of pure fluoric acid with ammonia; the third relates to fluoboracic acid; and the fourth to the compound of this acid with ammonia.

For obtaining silicated fluoric gas, the author mixes finely-powdered glass with the materials which, at the same time that it increases the product, saves the retort from the action of the acid. In order to discover the proportion of its constituent parts, the gas was transferred to a solution of ammonia, which gave a precipitate of 27·2 grains silica from 40 cubic inches of the gas, amounting to 61·4 per cent., the rest being mere fluoric acid. When this gas is united to water, it deposits a portion of its silica, but still retains more than 54 per cent.

By an experiment conducted with care, water was found to decompose about 263 times its bulk of this gas; but by subsequent trials it appeared capable of absorbing as much as 365 times its volume.

Though this gas, in combining with ammoniacal gas, requires twice its bulk for saturation, the proportion of their densities is such, that the acid exceeds the ammonia in weight more than 3 to 1.

In combining with dry ammonia, it retains the whole of its silica, and the compound may be sublimed unaltered; but when it is dissolved in water, it loses a part of its silica, and will deposit the whole by excess of ammonia, or by addition of other alkaline solutions. This subsilicated fluate of ammonia is likewise decomposed by sulphuric acid or by muriatic acid gas.

When the subsilicated fluate has been decomposed by an excess of ammonia, and evaporated in a vessel of platina, a simple fluate of ammonia is obtained; and it is estimated to consist of 76·4 ammonia, and 23·6 fluoric acid.

For the formation of fluoboracic acid gas, discovered by MM. Gay-Lussac and Thenard, Mr. Davy mixes powdered boracic acid, instead of powdered glass, with fluor spar and sulphuric acid: 100 cubic inches of this gas were found to weigh 73½ grains. Water condenses 700 times its bulk of this gas. Sulphuric acid absorbs about 50 times its bulk.

Equal volumes of this gas, and of ammoniacal gas, form a solid salt, that may be sublimed unaltered by a gentle heat. If another equal volume of ammonia be added, the whole becomes fluid, and will absorb still another equal portion of ammonia without apparent change. The excess of ammonia, however, is but loosely combined, and may be expelled by a gentle heat, or by simple exposure to the air.

On a Periscopic Camera Obscura and Microscope. *By* William Hyde Wollaston, *M.D. Sec. R.S.* Read June 11, 1812. [*Phil. Trans.* 1812, *p.* 370.]

The same views which the author originally had of the periscopic construction of spectacles, naturally suggested to him a corresponding improvement in the camera obscura. But though it is evident that enlargement of the field of view, and some improvement in the distinctness of images obliquely situated, might be made by causing each part of a lens to be as nearly as may be at right angles to each pencil of rays that are to be collected to a focus, it was not clear to what extent this principle could be carried where the portion of lens is necessarily large, and consequently includes parts that may require a different form for preserving distinctness of other pencils of rays.

He has, accordingly, made trial of different forms of meniscus for this purpose, and describes the construction which appears to possess the greatest advantage with least detriment to the brilliancy and distinctness of objects centrally situated. The radii of curvature here preferred are in the proportion of 2 to 1.

A meniscus so formed is placed with its convex surface towards the plane of representation, and with its concavity toward the objects.

The aperture of the lens itself is made as large as the tools will conveniently admit, but the pencil of rays admitted to it in each direction is limited by a diaphragm, placed between it and the objects at about one eighth of its focal length from the lens, and having an opening about one tenth or one eleventh of the focal length in diameter.

By means of a camera thus constructed, the author observes, that objects as far as 30° removed from its centre are represented with considerable distinctness on the same plane as objects centrally situated.

The author also describes a microscope which he terms periscopic, because it is formed on the same principle of endeavouring to place all portions of a lens at right angles to the pencils transmitted through it in different directions. In the microscope this is effected by placing two plane convex lenses on opposite sides of the same aperture in a piece of thin metal interposed between their plane surfaces. The central opening being by that means nearly in the centre of curvature, no rays are suffered to pass to the eye but what are at right angles to both surfaces; and a field of view of at least 20° is obtained with a greater degree of illumination than can be admitted in the usual construction.

The paper is accompanied by a diagram for determining, by inspection, the various combinations of radii that will give to a meniscus any required focal length.

Further Experiments and Observations on the influence of the Brain on the generation of Animal Heat. By B. C. Brodie, *Esq. F.R.S. Communicated to the Society for promoting the knowledge of Animal Chemistry, and by them to the Royal Society.* Read June 18, 1812. [*Phil. Trans.* 1812, *p.* 378.]

In some former experiments it was found, that in an animal from whom the head was removed, the circulation of the blood might be maintained by means of artificial respiration; that under these circumstances the blood underwent the usual changes of colour in the two capillary systems, and carbonic acid was evolved from the lungs, but no heat was generated, and the animal cooled more rapidly than a dead animal of the same size. In the present communication, some experiments are detailed, which were instituted with a view to the further elucidation of this subject.

An apparatus was constructed for the purpose of measuring the air consumed in respiration; and two series of experiments were instituted,—the first to ascertain the quantity of air consumed by animals breathing under ordinary circumstances; and the second to ascertain the quantity consumed by animals which are made to breathe artificially after the functions of the brain are destroyed. It was found, that in animals breathing under these different circumstances, there is little or no difference in the quantity of oxygen which disappears, and of carbonic acid, which is evolved in a given space of time; but that it uniformly happens, that where the functions of the brain are suspended, no heat is generated; and that the animal cools more rapidly than a dead animal.

In another experiment, an animal apparently dead from the poison of essential oil of almonds, was made to recover, by respiration being artificially produced until the poison had ceased to exert its influence. It was found, that while the functions of the brain were suspended, no heat was generated, but that as the sensibility of the animal was restored, the power of generating heat returned.

These experiments, as well as those detailed in the Croonian Lecture for 1810, go far towards proving that the temperature of warm-blooded animals is considerably under the influence of the nervous system; but, what is the nature of the connexion between them? Whether the brain is directly or indirectly necessary to the generation of animal heat? These are questions which, in the present state of our knowledge, must remain unresolved.

On the different Structures and Situations of the Solvent Glands in the digestive Organs of Birds, according to the nature of their Food and particular Modes of Life. By Everard Home, *Esq. F.R.S.* Read June 18, 1812. [*Phil. Trans.* 1812, *p.* 394.]

Since the solvent glands in birds are larger than in quadrupeds, Mr. Home has investigated their structure in different classes of birds, and has here collected several varieties that he has observed;

and his descriptions are accompanied with drawings of various peculiarities of structure.

Of the birds that live on animal food, the author has examined several kinds of falcon, the Soland goose, the crane, the cormorant, the common gull, and the snow-bird, the last of which differs considerably from the rest.

Among graminivorous birds, the swan and goose are noticed as differing from most others in the situation and structure of these glands. Others of this tribe here examined are the turkey, the cassowary, the American ostrich, and the African ostrich. In the three last the solvent glands are in a cavity of unusual size; and the muscular structure of the gizzard is uncommonly weak, which the author conceives to be connected with the mode of progressive motion, which is the same in these birds, and may serve to grind the food without the assistance of strong muscular action.

On some Combinations of Phosphorus and Sulphur, and on some other Subjects of Chemical Inquiry. By Sir Humphry Davy, *Knt. LL.D. Sec. R.S.* Read June 18, 1812. [*Phil. Trans.* 1812, *p.* 405.]

The author has formerly described to the Society two compounds, consisting of phosphorus and oxymuriatic acid, or chlorine; one of them is a solid compound; and by his present experiments, consists of three parts of phosphorus combined with twenty of chlorine. The second compound contains only half this quantity of chlorine, and is a fluid, having the specific gravity of 1·46. The fluid will also dissolve a still further quantity of phosphorus; but the author has not ascertained whether there is any definite limit to the proportion so dissolved.

When this fluid compound is treated with water, it forms a thick fluid, of the consistence of syrup, that crystallizes slowly by cooling. These crystals may be called hydrophosphorous acid; for by heat they are decompounded into phosphoric acid, and a peculiar gas, consisting of phosphorus and hydrogen. This gas differs from common phosphuretted hydrogen, in not being spontaneously inflammable when mixed with common air, and in being considerably less fetid than that gas. The proportions of its constituent parts are estimated to be four hydrogen, with twenty phosphorus. The phosphoric acid contained in the crystals, derived its origin from the water added to the original fluid; while the hydrogen of the water escaped with the chlorine in the form of muriatic gas.

In the interchange of elements which takes place in these combinations, there are many circumstances favourable to ascertaining the proportions in which the several elements unite with each other; and the author observes, the results accord remarkably well with other determinations.

In the same manner respecting sulphur, a similar accordance between the proportions, by weight, in which it unites with different elements, confirms other estimates of the elementary number fit to

express the relation of this body to hydrogen, the unit of the scale. For since in sulphuretted hydrogen the hydrogen is not altered in bulk by the sulphur it holds in solution, the increase of weight shows the proportion of sulphur combined. And so likewise in sulphurous acid gas, the bulk is not greater than that of the oxygen which it contains; and since the specific gravity is double that of oxygen, the increase proves that sulphur, in this instance, has combined with an equal weight of oxygen. When sulphur combines with a larger proportion of oxygen, the presence of water appears to be requisite as the medium of union. In the phosphoric acid, on the contrary, water is not necessary for uniting the full proportion of oxygen with which phosphorus is capable of combining.

With respect to the water that has been observed to be retained by many earths, and metallic precipitates, it is remarked, that this is probably combined with them in definite proportion, and that the presence of the water has considerable influence on their colours, and on other properties.

On a new detonating Compound: in a Letter from Sir Humphry Davy, *LL.D. F.R.S. to the Right Hon. Sir* Joseph Banks, *Bart. K.B. P.R.S.* Read November 5, 1812. [*Phil. Trans.* 1813, *p.* 1.]

The present account is intended as a caution to others against the dangerous effects of the very explosive detonation, by which the author has himself been a sufferer. His attention was first directed to the subject by a letter received from France, mentioning that about twelve months since, a compound had been discovered of azote with chlorine, which appears in the form of an oil heavier than water, and which explodes, by a gentle heat, with all the violence of the fulminating metals. The letter adds, that this discovery cost the operator an eye and a finger. Since the letter contained no account of the mode of preparing the compound, and as none could be found in any of the French journals, Sir Humphry Davy pursued a hint given him by Mr. Children, who informed him that his friend Mr. Burton had, in the month of July last, observed the formation of a volatile oily substance in a solution of nitrate of ammonia, exposed to chlorine in the state of gas. In repeating the experiment, the author perceived first an oily film on the surface of the fluid, which gradually collected into small globules, and fell to the bottom. One of these globules being taken out while floating on the surface of the water, and slightly warmed, exploded with brilliant light, but without any violence of detonation.

In a series of experiments carried on jointly with Mr. Children and Mr. Warburton, other ammoniacal solutions were substituted, and it was found that the same product was obtained by means of oxalate of ammonia, or by a weak solution of pure ammonia. The first instance that occurred of violent explosion, was in their endeavours to form larger quantities in a Wolfe's apparatus, by a series of bottles containing the different solutions.

The heat generated by the union of ammoniacal vapour and chlorine, caused the destruction of the whole apparatus by an instantaneous explosion.

From this time their attempts to form the oil were confined to small phials of the gas, and their trials of its properties were limited to a quantity not greater than a grain of mustard-seed; but still the results were attended with danger. In attempting to collect the gas produced in its explosion, by heating a very small quantity under water in a curved tube, the tube was shattered with great violence, and the author received a wound in the transparent cornea of one eye, from which he has not yet recovered.

Explosions equally violent were afterwards witnessed by Mr. Children and Mr. Warburton, even without confinement: when a small globule of the oil was thrown into a glass of olive oil, oil of turpentine, or naphtha, the glass, though strong, was in each instance shivered to pieces.

When a globule larger than a grain of mustard-seed was touched under water by phosphorus, the explosion was so violent as to break any glass vessel in which the experiment was made. But when smaller quantities were employed, a gas could be collected which, by the experiments hitherto made, contains no oxygen and no inflammable gas.

When thrown into the solutions of phosphorus, in ether, or alcohol, it detonates most violently; but neither ether nor alcohol alone exhibit any violence in their action upon it.

In muriatic acid it gives off gas rapidly, and disappears without explosion. Various experiments were also instituted with other substances, as sulphur and resin, among others, but without any remarkable effects.

From the general tenour of these experiments, the author thinks it probable that the substance here examined is a compound of azote and chlorine, formed by the decomposition of ammonia; while the hydrogen of the ammonia unites with another portion of chlorine, and forms muriatic acid.

The heat and light produced during the expansion of this oil into gaseous matter, is considered by the author to be without a parallel in our present collection of chemical facts; and the suddenness of the explosion more instantaneous than that of any compound hitherto known.

On a remarkable Application of Cotes's *Theorem. By* J. F. W. Herschel, *Esq. Communicated by* W. Herschel, *LL.D. F.R.S.* Read November 12, 1812. [*Phil. Trans.* 1813, *p.* 8.]

This communication includes an application of Cotes's theorem to conic sections in general; but the application noticed in the title relates to the parabola in particular, and it may be thus enunciated.

If any number of radii vectores S P be drawn from the focus to the curve, making equal angles P S P with each other; and if an equal

number of angles P S Q, Q S Q be also taken, each equal to M S P, the angle which the first drawn radius makes with the axis, then will the continued product of all the radii S P be equal to the last S Q multiplied by the latus rectum raised to the power of $n - 1$, n being the number of angles taken.

The author thence proceeds to deduce other theorems that would be for the most part complicated and unintelligible when geometrically enunciated, though sufficiently simple in their algebraic expressions. They are indeed, as the author observes, properties rather of the equations of the conic sections, than of the curves themselves; properties of a limited number of disjoined points, determined according to a certain law, rather than of a series of consecutive points composing a line.

In the course of this investigation the author employs one species of notation, which is new, and for which he apologizes, by explaining its advantage in point of simplicity.

Observation of the Summer Solstice, 1812, *at the Royal Observatory. By* John Pond, *Esq. Astronomer Royal, F.R.S.* Read November 12, 1812. [*Phil. Trans.* 1813, *p.* 27.]

Since a minute description of the new circular instrument, which has been lately put up at Greenwich, is intended to be given to the Society as soon as it is completed in every respect, the Astronomer Royal takes no further notice of its construction than is necessary to show by what means the results of his observations of the sun at the last solstice was obtained.

In other instruments, which take their point of departure from a plumb-line or level, the zenith distance of the sun is the primary object of investigation; and the polar distance of the sun, which is the ultimate object, is obtained by adding the co-latitude of the place, which completes the entire arc.

But by the mural circle at Greenwich, to which there is neither level nor plumb-line, the total arc may be measured without any exact knowledge of the zenith point; and the co-latitude, which in all other cases it is so essential to know correctly, becomes an object of mere curiosity, rather than of real necessity.

It is, however, convenient to assume some imaginary point near the zenith, the position of which, with respect to the fixed stars, may be determined within one tenth of a second; and from this imaginary point Mr. Pond measures the distances of the sun southward, and of the pole northward, as the best means of obtaining the entire arc; but he also adds a computation of the same solstitial place of the sun, as obtained by direct measurement from the pole without the aid of his imaginary intermediate point, and the difference is found to be only 0·15 of a second.

In the determination of this arc, it is evident that, however accurately it may have been mechanically determined, it must still be

subject to whatever uncertainty remains with regard to astronomical refraction.

As the Astronomer Royal has not been in possession of the instrument a sufficient length of time for deducing the real quantity of refraction from his own observations, he has hitherto employed those of Dr. Bradley, which have been many years in use at the Royal Observatory; but he observes, that any alteration which may be hereafter found necessary, may easily be made as correction to the above observations.

Observations relative to the near and distant Sight of different Persons. By James Ware, *Esq. F.R.S.* Read November 19, 1812. [*Phil. Trans.* 1813, *p.* 31.]

The author states, in the first place, that he has rarely observed short-sightedness in children under ten years of age, and that he considers it as commencing generally between that period and eighteen; that it at first occasions so little inconvenience, that it is not noticed by those who have not access to concave glasses, and consequently is very frequently overcome by the natural efforts of the eye. Persons, on the contrary, in the higher ranks of society, who have it more in their power to indulge a slight weakness, by having recourse to short-sighted glasses, soon confirm the imperfection, and in many instances even render it worse, by employing glasses deeper than are necessary.

For the purpose of instituting a comparison between the proportional prevalence of this defect in different classes of society, Mr. Ware made inquiry in the three regiments of Foot Guards, containing nearly 10,000 men, and also in the two Universities, Oxford and Cambridge. In the Guards short-sightedness among the privates is scarcely known; and not more than half a dozen recruits are said to have been rejected for this imperfection in the course of twenty years. In the Universities, on the contrary, the numbers are so considerable, that in one of the colleges in Oxford, it is said that of 127 persons, so many as thirty-two have used either a hand-glass or spectacles. It is thus proved that short-sightedness is corrected in one class of persons, and encouraged in the other; and it is evident that even in those who absolutely require glasses, it may be increased by using such as are deeper than are really necessary, or counteracted by employing the lowest with which the eye can see with comfort.

It is observed, however, that extreme short-sightedness sometimes occurs in children from visible imperfection in the form of the cornea, and sometimes arises as an accidental and temporary consequence of general debility, and is then removable by chalybeate medicines and bracing applications.

Dr. Porterfield has observed, that the pupils of short-sighted persons are in general more dilated than those of others; but Mr. Ware does not admit this to be generally the case; neither does he admit

the common opinion to be well founded, that the magnitude of the pupil varies according to the distance of the object viewed. He names, however, one extraordinary instance of a lady, whose pupil contracts only when she views objects at the distance of nine inches, but at other times is dilated very nearly to the full extent of the rim of the cornea.

Mr. Ware has also made experiments similar to those of Dr. Wells, on the alteration of the power of the eye during that dilatation of the pupil which is produced by the external application of belladonna. Those of the author were attended with the same result of lengthening the focus of either eye to which the belladonna was applied, without affecting the customary range of vision in the other eye.

The author observes, that short-sightedness does not depend on the greater or less concavity of the cornea alone; since its distance from the retina, and the convexity of the crystalline also, must be taken into the account.

It has sometimes been observed, that old persons have in a short time recovered the perfect sight of younger persons; and this has been explained by Dr. Porterfield, by a supposition of the absorption of fat from the bottom of the orbit, allowing the axis of the eye to become more elongated: but Mr. Ware thinks it more likely to have arisen from absorption of the vitreous humour, in consequence of which the sclerotica would be pressed inwards, and that then the axis of the eye would be elongated.

Mr. Ware observes, that persons in general who use glasses, possess the power of seeing both near and distant objects with the same glass, but that this is not the case with those who have been couched, who always require a different glass to enable them to see distant objects; proving thereby that the adapting power of the eye depends on the presence of the crystalline. In consequence of this defect, such persons judge very imperfectly of distances.

In comparing the range of adaptation possessed by short-sighted persons with that of others, the author conceives the range of the former to be much less; his estimate being made by the number of inches through which the range of distinct vision extends.

The author, being himself very short-sighted, remarks, that the change that has taken place in his own eyes by age, does not accord with the observation generally made, that short-sighted persons become less so as they advance in life. In his eyes the shortest distance of distinct vision remains nearly where it was; but the power of discerning distant objects is so far lessened, that for this purpose he requires a glass one degree deeper than that which he commonly employs, and with which he formerly used to distinguish distant as well as near objects; and as he is acquainted with other instances in which a correspondent change has taken place, he is of opinion that such changes are by no means unfrequent. However, in two of those here enumerated, this change was produced by evident disease; and in one of them it was only temporary. A third instance mentioned of an eye becoming less long-sighted, is occasioned by unusual efforts

of adaptation. An instrument-maker, by employing a microscope, for the purpose of dividing with accuracy for several days together, is afterwards able to read without spectacles for a few weeks, but his sight then gradually elongates, till he again has occasion to employ himself in dividing.

Two other cases are also mentioned, of long-sightedness reduced to vision at a moderate distance, both arising from disease, and both speedily relieved by evacuating remedies.

The Bakerian *Lecture. On the elementary Particles of certain Crystals. By* William Hyde Wollaston, *M.D. Sec. R.S.* Read November 26, 1812. [*Phil. Trans.* 1813, *p.* 51.]

In this lecture the author undertakes to explain a difficulty that has occurred in crystallography, respecting the primitive molecule of those bodies that assume the octohedral and tetrahedral forms, when broken in the direction of their natural fractures.

The substance that he selects as most convenient for experiment is fluor spar, which may very readily be divided into any number of acute rhomboids, having the angles of their surfaces 60° and 120°.

These might be regarded for all the practical purposes of crystallography as the integral molecule, and from thence all the other modifications of these solids might very simply be derived.

But it is observed, that each acute rhomboid thus obtained may be again split in a new direction at right angles to its axis, so that a tetrahedron may be detached from each extremity, leaving from between them a regular octohedron.

Consequently this rhomboid cannot be considered as the primitive, and we are left in doubt not only which to prefer of the two last-named solids, but even whether either of these can be primitive; since no possible arrangement of tetrahedra alone, or of octohedra alone, will fill any space without leaving vacuities.

The author having observed that both these forms would result from the arrangement which spheres would most naturally assume by mutual attraction, proposes a theory founded on that observation, which he thinks is not liable to objection, and endeavours to extend this hypothesis, by showing that with some modifications a corresponding theory may be applied to other forms well known to crystallographers.

With regard to the triangular arrangement of balls in a plane, and their tetrahedral grouping in solidity also, he finds that he has been anticipated by that universal genius Dr. Hooke: but he observes, that Dr. Hooke's ideas upon this, as upon many other subjects, are but imperfectly developed; and that he takes no notice of the octohedral group, formed by placing four balls in a square, with one above and one beneath them. Accordingly, Dr. Hooke could know nothing of that which forms the principal novelty of the present observation, namely, that when a mass of spheres has been formed wholly according to the triangular or tetrahedral arrangement, then certain sections

of the same mass present also the square arrangement, and accordingly certain portions of it exhibit the octohedral group.

The author remarks, in support of this theory, that a large proportion of those substances which assume the octohedral form, are considered by chemists as simple bodies, and are therefore more likely to have the simple form of spheres than such as consist of more than one element. Since the supposition of spherical particles appeared to him to afford so satisfactory an explanation of an acknowledged difficulty in crystallography, he was led to consider what other forms would result from the union of solids most nearly allied to spheres; and he observed that obtuse rhomboids, like those of carbonate of lime and other substances, would be formed by the union of oblate spheroids, as indeed Huyghens had long since observed; and that by the union of oblong spheroids, the natural result would be triangular and hexangular prisms, as are found in beryl and phosphate of lime.

But the most singular arrangement noticed, is that which affords an explanation of the origin of cubes in crystallography. These, he supposes, may consist of spherical particles, of two different kinds, regularly intermixed in equal numbers (in conformity to the most recent views of binary combination in chemistry); for these, he observes, will not tend, as before, to the octohedral arrangement, but will be perfectly in equilibrio when every group of eight balls composes a cube, according to the most obvious course of alternation of the two elements. For in that case all similar balls will be equidistant from each other, and will also be equally distant from all adjacent balls of the opposite denomination.

In a note are subjoined some observations on a theory of crystallization proposed by M. Prechtl, who imagines that a mass of soft spheres may all be compressed into tetrahedra, which is demonstrably impossible. That by another degree of softness or of attraction, spheres, each surrounded by five others, may be compressed into triangular prisms, without regard to the different degree of compression that must take place in the direction of the axis; that other spheres again less compressible than before, and consequently surrounded by as many as six others, may be formed into cubes, which indeed is admitted to be a very possible supposition.

It is observed, however, that M. Prechtl denied that a sphere can be surrounded by more than six, although, in fact, the most probable supposition is, that each soft sphere would be surrounded by twice that number, and would form a mass of regular dodecahedra.

On a Substance from the Elm Tree, called Ulmin. By James Smithson, *Esq. F.R.S.* Read December 10, 1812. [*Phil. Trans.* 1813, *p.* 64.]

The substance here examined by the author, we are told, was first made known by the celebrated Klaproth. It has been ranked as a distinct principle, soluble in water, but insoluble in alcohol or ether, and convertible, by the action of nitric or oxymuriatic acids, into a

resinous matter no longer soluble in water, but now rendered soluble in alcohol, by a supposed union with oxygen derived from these acids.

Mr. Smithson being in possession of ulmin, sent to him from Palermo by the same person who had furnished M. Klaproth with the subject of his researches, has made various experiments, which lead to a different opinion of its nature from that which has been entertained.

When ulmin is dissolved in water, a dilute solution is yellow; but when concentrated, it is of a dark red, like blood. This solution slowly and feebly restores the colour of turnsol, after it has been reddened by an acid. Most acids occasion a copious precipitate from this solution of the matter which has been considered as resin. The solution, however, still retains a slight yellow colour, from a small quantity of this matter which remains dissolved. By evaporation of the solution a salt is obtained, consisting of potash combined with the acid employed in the experiment; and the quantity of potash, by various trials, amounted to about one fifth part of the weight of the ulmin.

The precipitate, when dried, is very glossy, and has a resinous appearance. In minute fragments it is found to be transparent, and of a deep garnet colour. It burns with flame, and is reduced to a white ash.

Alcohol does dissolve it, but very sparingly.

Water also dissolves a small quantity, and the solution seems to redden turnsol. Neither ammonia nor carbonate of soda promote its solution in water; but a small quantity of potash dissolves it immediately, and abundantly, and appears to regenerate ulmin, with all its original properties.

Hence Mr. Smithson infers, that ulmin is not a simple vegetable principle of anomalous qualities, but a combination of potash, with a matter more nearly allied to the extractives than to the resins.

The author has also investigated the properties of a substance obtained from the elm-tree in this country, which differed from that of Palermo in containing a redundant quantity of potash in the state of carbonate. He also made experiments on the sap of the elm-tree, from which, however, he did not succeed in obtaining ulmin.

On a Method of Freezing at a distance. By William Hyde Wollaston, *M.D. Sec. R.S.* Read December 17, 1812. [*Phil. Trans.* 1813, *p.* 71.]

The method here described by the author, is performed by means of an instrument, to which he gives the name of Cryophorus, expressing its office of frost-bearer. It consists of a tube, which may be two or three feet long, or even more, terminated by a ball at each end. One of these balls contains a small quantity of water to be frozen, and the rest of the instrument is as complete a vacuum as can be obtained.

In making this instrument, one of the balls terminates in a capillary

tube; and when water has been admitted into the other, it is boiled over a lamp till all the air is expelled; and while the stream is still issuing with violence through the capillary extremity, the end of it is held in the flame of the lamp, till in proportion as the force of the steam diminishes, the heat acquires power to seal the tube hermetically.

When such an instrument has been successfully exhausted, if the empty ball be placed in a freezing mixture of salt and snow, the water contained in the opposite ball will be frozen solid in a very few minutes.

The first vapour being condensed by the common effect of cold, is immediately succeeded by a fresh emission, with proportional reduction of temperature; so that heat is continually withdrawn, or cold generated at a distance by the freezing cause.

A Catalogue of North Polar Distances of some of the principal fixed Stars. By John Pond, *Esq. Astronomer Royal, F.R.S.* Read December 17, 1812. [*Phil. Trans.* 1813, *p.* 75.]

A Description of the solvent Glands and Gizzards of the Ardea Argala, the Casuarius Emu, and the long-legged Cassowary from New South Wales. By Sir Everard Home, *Bart. F.R.S.* Read December 17, 1812. [*Phil. Trans.* 1813, *p.* 77.]

In the first of these birds the solvent glands are different from those of any other bird examined by the author, each gland being made up of five or six cells, that open into one common excretory duct; but its gizzard is very similar to that of the crow.

In the Emu the solvent glands are oval bags, one fourth of an inch in length, and one sixteenth wide. The gizzard differs from that of the crow in having a thicker lining, and is remarkable solely for its situation; for it is not placed, as usual, between the stomach and the duodenum, but forms a pouch on one side, so that food can pass onwards direct into the duodenum, without being received into the gizzard.

In the Cassowary of New South Wales, the solvent glands are similar to those of the emu, but larger; and the gizzard is also similar in every respect, but stronger.

The author further remarks upon the circumstances in the structure of the cassowaries, and other birds most nearly allied to them, which adapt them to the different degrees of fertility of the countries they inhabit.

The Emu of Java, where there is abundance of food, has intestines that are of large diameter, and comparatively short, so as to afford free passage to the superfluity of food they take, and a gizzard to be employed only occasionally.

The Cassowary of New South Wales has intestines of smaller diameter, thirteen feet long; and a stronger gizzard, more frequently employed in a less productive country.

The *Rhea Americana*, with intestines equally long, has a gizzard so placed, that no part of the food can escape trituration.

And lastly, the Ostrich of Africa, where its means of subsistence are most precarious, has a gizzard extremely strong, and intestines seventy feet in length.

Additional Remarks on the State in which Alcohol exists in fermented Liquors. By William Thomas Brande, *Esq. F.R.S.* Read December 17, 1812. [*Phil. Trans.* 1813, *p.* 82.]

The question here discussed, is whether alcohol exists in fermented liquors ready formed as the result of fermentation, or is formed subsequently from them by the process of distillation. The latter the author has imagined to be the commonly received opinion; and in a preceding communication to the Society he endeavoured to refute it, by showing that the same quantity of alcohol was always obtained, whether the distillation was performed at a higher or lower temperature. Since the conclusions which he then drew may be objected to on the ground that even the lowest temperature that he employed for distillation might be sufficient to give a new arrangement to the elements, and thereby form alcohol, he now employed a totally different process, in which distillation is altogether avoided.

Having observed that the unsuccessful attempts which had been made to separate alcohol from wines by subcarbonate of potash, and from which some persons had inferred its non-existence, appeared to fail in consequence of the union of the alkali with the colouring, extractive, and acid matters contained in the liquor, the author endeavoured, and has succeeded, in effecting a previous separation of these substances from wine, by means of subacetate of lead, or subnitrate of tin.

When these are added, a dense and copious precipitate is instantly formed; and if the liquor be then filtered, it passes perfectly colourless, containing alcohol, water, and a portion of the acid of the metallic salt employed.

By adding to this liquor half its quantity of subcarbonate of potash, nearly the whole of the alcohol that was contained in the wine is separated.

In Port wine thus treated, the quantity of alcohol was found to be 22¼ per cent., agreeing very nearly with former results by distillation.

To these inferences respecting the apparent proportion of alcohol in Port and in other wines, it has been objected, that they do not possess a power of intoxicating equal to such a mixture of alcohol and water.

Mr. Brande's reply to this objection is, that it requires some time for a mixture of alcohol and water to become incorporated; and that in this state of imperfect union it is warmer to the taste, and apparently more heating in its effects than when sufficient time has been allowed for their mutual penetration.

On a new Variety in the Breeds of Sheep. By Colonel David Humphreys, *F.R.S. In a Letter to the Right Hon. Sir* Joseph Banks, *Bart. K.B. P.R.S.* Read January 14, 1813. [*Phil. Trans.* 1813, *p.* 88.]

The breed here described by Col. Humphreys, first occurred as an accidental variety in the State of Massachusetts, about sixteen miles distant from Boston, in the year 1791; a lamb was produced, having a singular appearance, from the shortness of its legs; and by some chance rather than by any particular design, was reserved for breeding. In the first season only two lambs were yeaned, resembling the father; but in subsequent years several more were produced, having the same peculiarities; and a breed was thence established, which, from the shortness of their legs, and comparative length of back, were at first called the Otter breed; but from a surgeon who afterwards dissected one of them, they were called Ancon, from the elbow-like crookedness of the fore-leg.

Experiments in crossing have in very few instances mixed the qualities of this breed with those of others; but the singularity of form is so far established, that when both parents are of the true otter or ancon breed, the descendants inherit, almost without exception, their peculiar appearance and properties of form.

When an ancon ewe is impregnated by a common ram, the progeny resembles wholly either the ewe or the ram; and the same is the consequence of breeding from the common ewe with an ancon ram.

The most obvious difference between the young of this and of other breeds, consists in the shortness and crookedness of their legs, by which, as well as by some general debility of constitution, they become cripples as they advance in age. The contrast is very striking when a common ewe has twins by an ancon ram, when it sometimes occurs that one is ancon and the other common; so that one short-legged and one long-legged lamb are seen sucking the same dam at the same time.

In the fleece of the true ancon there seems to be nothing decisively different from common; but in the ancon-merino, by which is meant the offspring of the merino ram from the ancon ewe, though the shape of the progeny be exactly that of the ewe, its fleece partakes strongly of the silky feel of the merino wool.

When ancons are put into the same inclosure with other sheep, they are observed to keep together, separate from the rest of the flock.

Although they arrive at maturity somewhat later, they are said to live as long as other sheep; but in consequence of their deformity they can neither run nor jump, and move with a very awkward gait, having their fore-legs always crooked, and their feet turned inwards.

The principal reason for propagating a breed with such appearance of imperfection, has been the advantage that arises from their incapacity to leap over fences. In that part of the United States where

they have been encouraged, there are few commons, no hedges, no shepherds, no dogs to attend the flocks; and the fences of wood and stone are not sufficient to prevent active sheep from breaking pasture, to the great destruction of adjacent crops.

The advantage, however, in this respect, is counterbalanced by a corresponding inconvenience arising from the same source, namely, the great difficulty of driving such cripples to market, at the same time that they are generally not so fat as others, from the greater labour they undergo in gathering their food.

These objections are indeed such, that since the introduction of Merinoes, which are equally gregarious, quiet, and orderly, in addition to the strong recommendation of their fleeces, the ancon breed appears in danger of becoming wholly extinct; so that the author had some difficulty in procuring one in Boston to be dissected, for the purpose of sending a skeleton, which accompanied the letter, and was laid before the Society.

Experiments to ascertain the coagulating Power of the Secretion of the gastric Glands. By Sir Everard Home, *Bart. F.R.S. Communicated by the Society for promoting the Knowledge of Animal Chemistry.* Read January 21, 1813. [*Phil. Trans.* 1813, *p.* 96.]

It has long since been observed, that the first step in the process of digestion is the conversion of the food into a jelly; but whether this is effected by means of the gastric liquor alone, or by a joint operation of other secretions, has not been ascertained. From Mr. Hunter's experiments, it appeared that the same species of coagulation takes place in the same food admitted into the stomach of a great variety of animals; and that in the calf's stomach this power resided in the fourth cavity alone; since the mucus taken from the surfaces of the first, second, or third cavities, had no such effect as rennet, which is prepared by infusion of the inner membrane of the fourth cavity.

The same inquiry is here pursued by the author, with a view to ascertain more accurately what part it is that possesses this property in the highest degree, by comparison of the effects of rennet prepared from different portions.

By this mode of trial no part of the hog's stomach was found to coagulate milk, but that near the pylorus, where the gastric glands are situated.

Experiments were next made with rennets prepared from the crop and gizzard of a cock, from the stomach of a shark, the stomach of a salmon, and that of a thornback, all of which had the power of coagulating milk.

Other experiments were afterwards made, with the assistance of Mr. Hatchett and Mr. Brande, on the comparative powers of different parts of the same stomach, and the difference in various species of animals, the chicken, hawk, turkey, and calf.

In a chicken the horny lining of the gizzard gave a firmer curd

from milk than the cardiac portion of the stomach. The cardiac portion of the stomach of the hawk was found more powerful than the same part of a common fowl.

The gastric glands were carefully dissected out from behind the membrane that lines the cardiac extremity of the stomach of a turkey; and of these, forty grains, by weight, were taken, and their effect compared with an equal weight of membranous lining of the same cavity, an equal weight of membrane from the fourth cavity of a calf's stomach in a recent state, and forty grains of dry rennet. Since the last must have been prepared from about four times its weight of recent membrane, its effect was produced in much the shortest time. The coagulation effected by the gastric glands took place nearly at the same time as by the recent calf's stomach; while that from the lining of the turkey's stomach was nearly three times as long in producing the corresponding effects.

From these experiments, the author infers that the secretion from the gastric glands possesses the power of coagulating milk, and communicates that property to adjacent parts, by which it is imbibed.

On some Properties of Light. By David Brewster, *LL.D.F.R.S.Edin. In a Letter to Sir* Humphry Davy, *LL.D. F.R.S.* Read January 28, 1813. [*Phil. Trans.* 1813, *p.* 101.]

The author, having been for some time past engaged in a course of experiments on the refractive and dispersive powers of different substances, the details of which are intended for future publication in a separate work, confines himself, at present, to a relation of such of his results as have most of novelty or importance. After repeating the experiments that have been made by others on the properties that light acquires by transmission through Iceland-spar, and upon the corresponding properties of reflected light originally discovered by Malus, and by him termed polarization, Dr. Brewster observed a singular appearance of colour on each side of a luminous object, viewed through a thin slice of laminated agate. Upon examination of these coloured images through a prism of Iceland-spar, this light was found to be similarly polarized, so as to appear or disappear accordingly as the laminæ of the agate were parallel or transverse to the principal section of the spar. He found also that the colourless light transmitted directly through the agate, and from which the coloured rays had been separated, was polarized as well as the coloured rays, appearing and disappearing alternately with them during the revolution of the spar. And accordingly when light previously polarized by reflection was received upon the agate, its transmission or reflection depended on the relative position of the laminæ of the agate to the plane of reflection; for when these were at right angles to each other, no light whatever was transmitted.

In the same manner light polarized by transmission through the laminated agate, manifested the usual properties of light so affected by other means.

Along with the polarized light, Dr. Brewster also observed a faint nebulous light not polarized, which he also finds in transmission through cornelian and chalcedony, and thinks it important as leading to a satisfactory theory of polarization.

The next observation of the author relates to the high refractive power of chromate of lead, which, he remarks, is greater than that of any other body hitherto recorded; and upon its double refraction, which, he says, is so enormous, that the deviation of the extraordinary ray is more than thrice that produced by Iceland-spar.

The index *of refraction* assigned by Dr. Brewster *to chromate* of lead, is 2·926, and along with it he names realgar, of which the index is 2·510, as another substance that refracts more strongly than the diamond. Phosphorus, he adds, stands higher than has been supposed, being 2·224, and then native sulphur next in order 2·115.

The dispersive power of chromate of lead is observed to exceed that of other substances in a still greater proportion than its refractive power, being more than ten times as great as that of the densest flint-glass, and fifteen times as high as that of water.

The concluding section of the author's letter relates to the existence of two dispersive powers in all doubly refracting media. Mr. Cavallo, and others, have already observed, that the dispersions occasioned by the two refractions of Iceland-spar are not equal. Dr. Brewster observes that this is general to all, and he undertakes to assign the proportion of these two powers in different substances.

In chromate of lead the dispersive power manifested in the rays ordinarily refracted, is double that of rays obliquely refracted; and in Iceland-spar the disparity is nearly as great.

The existence of a double dispersive power, it is observed by the author, instead of assisting in the explanation of other properties, only adds one to the numerous difficulties that are to be surmounted in reducing to any general rules those capricious phenomena exhibited by light in its passage through transparent bodies.

An Appendix to Mr. Ware's *Paper on Vision. By Sir* Charles Blagden, *F.R.S.* Read February 4, 1813. [*Phil. Trans*. 1813, *p*. 110.]

The author remarks, that Mr. Ware's observations with regard to short-sightedness, being in general merely the consequence of habit acquired at an early age, is conformable with his own experience in general, and that he himself is a particular instance of natural long-sightedness gradually converted into confirmed short sight. He very well remembers first learning to read, at the common age of four or five years, and that at that time he could see the usual inscriptions across a wide church; but that at the age of nine or ten years he could no longer distinguish the same letters at the same distance, without the assistance of a watch-glass, which has the effect of one slightly concave. In a few years more the same glass was not sufficiently powerful; but yet his degree of short-sightedness was so inconsiderable, that he yielded to the dissuasion of his friends from

using the common concave glasses till he was upwards of thirty years of age, when No. 2 was barely sufficient; and he very shortly had recourse to No. 3. In the course of a few years an increase of the defect rendered it necessary for him to employ glasses still deeper, and his sight soon required No. 5, where it has remained stationary to the present time. From the progress which Sir Charles Blagden has observed in his own short-sightedness, he is of opinion that it would have been accelerated by an earlier use of concave glasses, and might have been retarded, or perhaps prevented altogether, by attention to read and write with his book or paper as far distant as might be from his eyes.

In this communication he takes the same opportunity of adding an experiment made many years since on the subject of vision, with a view to decide how far the similarity of the images received by the two eyes contribute to the impression made on the mind, that they arise from only one object. In the house where he then resided, was a marble surface ornamented with fluting, in alternate ridges and concavities. When his eyes were directed to these, at the distance of nine inches, they could be seen with perfect distinctness. When the optic axes were directed to a point at some distance behind, the ridges seen by one eye became confounded with the impression of concavities made upon the other, and occasioned the uneasy sensation usual in squinting. But when the eyes were directed to a point still more distant, the impression of one ridge on the right eye corresponded with that made with an adjacent ridge upon the left eye, so that the fluting then appeared distinct and single as at first, but the object appeared at double its real distance, and apparently magnified in that proportion. Though the different parts of the fluting were of the same form, their colours were not exactly alike, and this occasioned some degree of confusion when attention was paid to this degree of dissimilarity.

A Method of drawing extremely fine Wires. By William Hyde Wollaston, *M.D. Sec. R.S.* Read February 18, 1813. [*Phil. Trans.* 1813, *p.* 114.]

The author refers to Musschenbroek for an instance of a gold wire, recorded to have been drawn by an artist at Augsburg so fine, that one grain of it would have the length of 500 feet. It is not said how this was effected, and some doubt has been entertained of the possibility of it; but the author of this paper shows how gold may be drawn to the same degree of fineness, and also that platina may be made with great facility much finer than is above described.

The general principle of the method is the same for both. The metal intended to be drawn is first reduced, in the common mode, to a wire of about $\frac{1}{100}$th of an inch in diameter; and it is then coated with silver, so as to form a rod of considerable thickness. The rod is then drawn, as usual, till it is reduced to a slender wire, and it is presumed that the gold or platina contained in it is reduced in the

same proportion as the silver. By steeping for a few minutes in nitrous acid the silver is then dissolved; but the gold or platina remain unaffected, and require merely to be washed in distilled water in order to free them from any portion of the solution or other little impurities that may adhere during the solution.

The method employed by the author for coating gold wire is attended with more difficulty than he expected. A rod of silver having been previously drawn of considerable thickness, a hole was drilled through it longitudinally, and into this hole a gold wire was inserted so as to fill the hole. But in consequence of the toughness of fine silver, the operation of drilling was found extremely difficult, and this method was afterwards abandoned. It was found that platina might be advantageously substituted for gold, as in that case the first drawn wire might be coated with silver by fixing it in the axis of a cylindrical mould, and then pouring melted silver to fill the mould. The platina employed for this purpose was fused by the flame of a spirit lamp impelled by a current of oxygen, as contrived by Dr. Marcet: this platina having then been drawn alone to a wire $\frac{1}{103}$ of an inch in diameter, it received a coating of silver that was just 80 times the thickness of the platina: accordingly when the silver was reduced by drawing to $\frac{1}{100}$ of an inch in diameter, that of the platina was $\frac{1}{10000}$; but nevertheless it remained surprisingly tenacious in proportion to its substance. The greatest relative tenacity is however thought to have been at about $\frac{1}{18000}$ of an inch, which supported 1¼ grain before it broke. Accordingly this wire admitted being drawn considerably finer, and the author has even obtained portions as slender as $\frac{1}{30000}$ of an inch; but these were only in very short pieces, being in many places interrupted so that he could place no reliance upon any trials of their tenacity.

Some precautions are added respecting the method of freeing these wires from their coating of silver, with the recommendation of some little contrivances which the author has found convenient in handling objects so liable to be injured.

Description of a single-lens Micrometer. By William Hyde Wollaston, *M.D. Sec. R.S.* Read February 25, 1813. [*Phil. Trans.* 1813, *p.* 119.]

The author, being unable to measure some of his very small wires so accurately as he wished by any means at present in use, contrived the method here described, which he recommends as fully answering his expectations.

A lens having its focus at one twelfth of an inch is mounted in a plate of brass, and by the side of it is made a small perforation, as near to its centre as $\frac{1}{16}$th of an inch.

When a lens thus mounted is placed before the eye for the purpose of examining any small object, the eye can at the same time see distant objects through the adjacent perforation, by reason of the magnitude of the pupil, which is sufficient for receiving rays through

both the lens and the naked aperture. The magnified object may thus be compared with a scale of any large dimensions at such a distance as may best suit the convenience of the observer.

The author, however, recommends a small scale attached to the instrument, as better adapted for steady comparison with the object to be measured.

The instrument has externally the appearance of a telescope, consisting of three tubes, with the little lens at its smaller extremity; and in the place of the object-glass is fixed the scale of equal parts, which consists of pieces of wire placed side by side, and so proportioned in their lengths at regular intervals, as to be easily counted.

A wire of known dimensions, as for instance, $\frac{1}{200}$th of an inch, being then placed in a suitable position before the lens, the tube is drawn out till this wire apparently occupies fifty divisions upon the scale, and consequently each division at that distance corresponds to $\frac{1}{50 \times 200}$ of an inch in the focus. Again, at half that distance the same wire covers only twenty-five divisions, each of which now corresponds with $\frac{1}{5000}$th of an inch seen in the focus of the eye-glass.

These numbers are marked accordingly on the outside of the tube, and the intermediate fractions $\frac{1}{8000}$, $\frac{1}{7000}$, &c. are found by dividing the exterior scale into equal intervals. Hence in the measurement of any wire, the number of divisions which it occupies on the interior scale are to be noted as numerator, and the number marked on the tube externally as denominator of a fraction, expressing its dimensions in proportional parts of an inch. Since the correctness of the instrument depends on the precision with which the first wire is known as basis of the exterior indications, the wire is made of fine gold, and its dimensions determined by the weight of a given length.

Observation of the Winter Solstice of 1812, *with the Mural Circle at Greenwich. By* John Pond, *Esq. Astronomer Royal, F.R.S.* Read February 25, 1813. [*Phil. Trans.* 1813, *p.* 123.]

The weather was so extremely unfavourable, that it was not possible to obtain more than eight observations of the sun, from which the obliquity of the ecliptic at the late solstice could be deduced; from these it is inferred to have been 23° 27′ 47″·35, that from the summer solstice having been 23° 27′ 51″·3. This small discordance, it is observed, might be easily made to disappear by a slight modification of Bradley's refractions; but the Astronomer Royal has not yet had an opportunity of making a sufficient number of observations on circumpolar stars with the new circle, to warrant making any corrections in his table of refractions, and he leaves the subject of the discordance of the solstices for discussion in a separate paper.

On the Tusks of the Narwhale. *By Sir* Everard Home, *Bart. F.R.S.* Read February 18, 1813. [*Phil. Trans.* 1813, *p.* 126.]

The author acknowledges himself indebted to the laudable zeal of Mr. Scoresby, jun. of Whitby, for the greatest part of the information which he here lays before the Society. Although the tusk of this animal is not uncommon, its skull has very rarely been brought into this country; and hence there has been little opportunity to correct the erroneous account given by travellers on this subject, who have generally maintained that the perfect narwhale has two of these tusks, although it is very common for one of them to be broken off. This opinion respecting the existence of two tusks has gained a more general belief in this country, from the exhibition of a stuffed narwhale for many years in the Leverian Museum, but in which it is observed that the second tusk was artificially fastened in its place.

The fact, says the author, is, that there is never more than one tusk in the full-grown narwhale, and this is always in the left socket; but there is also observable, on the right side, another socket, in which it is presumed that the milk-tusk had been contained, and afterwards shed.

A further observation of Mr. Scoresby's on this subject is, that the tusk of this animal is confined to the male, and consequently will not serve for a distinctive character of the species, as has hitherto been supposed.

A drawing of a female skull given to Sir Everard Home by Mr. Scoresby accompanies this communication. Simple inspection of this skull is represented by the author as sufficient to satisfy all doubts upon the subject, as there is no place provided for an adult tusk, although in all other respects it resembles that of the male, excepting that the milk-tusk appears to have been placed on the left side instead of the right.

Along with the drawing of the female skull is a representation also of a male skull now in the Hunterian Collection: and upon comparison of them, it is observed that that of the female appears broader in proportion to its length than that of the male, for want of that prominence in the fore part that supports the tusk of the male, which, it is observed, was in this instance five feet long, although it is evident, from the state of the sutures, that the animal had not attained its full growth.

Many other instances are well known to naturalists, of tusks confined to the male of several species, as in the horse; but since the elephant is the only animal that can in bulk and proportional size of tusk be compared with the narwhale, and since the female elephant has tusks as well as the male, analogy had not suggested a doubt concerning the existence of them in the female narwhale; and hence the observation of a fact that could not otherwise have been ascertained becomes proportionally interesting.

An Account of some organic Remains found near Brentford, Middlesex. By the late Mr. William Kirby Trimmer. *Communicated in a Letter from Mr.* James R. Trimmer *to the Right Hon. Sir* Joseph Banks, *Bart. K.B. P.R.S.* Read March 4, 1813. [*Phil. Trans.* 1813, *p.* 131.]

The greatest part of this account had, in fact, been drawn up by Mr. William Kirby Trimmer himself, for the purpose of communicating it, through the President, to the Society; so that very little has been added to what he had written, excepting the descriptive explanation of some sketches of several bones and teeth that accompany the paper.

The specimens had been collected at different times from two fields at some distance from each other, that have been dug for the purpose of making bricks. The first of these fields is about half a mile north of the Thames at Kew Bridge, and its surface about twenty-five feet above low water-mark. The first six or seven feet are sandy loam, rather calcareous towards the bottom, but containing no organic remains. The next stratum is sandy gravel, a few inches thick, containing shells of snails and of fresh-water fish, and a few bones of land animals. Under this is loam of variable thickness, from one to five feet, containing horns, bones, and teeth of oxen and of deer, with some shells also of snails and of fresh-water fish. The fourth stratum is gravel, from two to ten feet in thickness, covered at its surface with occasional thin patches of peat, and always thickest at those parts, and inhaling a disagreeable muddy odour. In this stratum were found teeth and bones of both the African and Asiatic elephant, of the hippopotamus, with bones, horns, and teeth of oxen. One tusk of an elephant measured as much as nine feet three inches, but was broken into small pieces in attempting to remove it. The fifth and main stratum, which follows, is the same blue clay which passes under London and its vicinity to the depth of two hundred feet and more.

This contains many detached nodules of pyrites, principally at the depth of about twenty feet from its surface, and many of them of considerable size. The extraneous fossils in this stratum are entirely marine, with the exception of some fruits and pieces of wood, which, however, appear to have been in the sea, as they are always pierced with Teredines. The shells are those of Nautili, Oysters, Pinnæ marinæ, and Crabs, with a great variety of smaller shells, and some teeth and bones of fish.

The second field mentioned by Mr. Trimmer is about a mile to the westward of the former, and at the distance of a mile from the Thames, from which it is elevated about forty feet. The first stratum here is sandy loam, to the depth of eight or nine feet, without any appearance of organic remains. Next lies sand, varying in depth from three to eight feet, and in coarseness from fine sand to sandy gravel at its lowest part. In this coarsest part are found a considerable quantity of teeth and bones of the hippopotamus and of the elephant; horns, bones, and teeth of deer and of oxen; with shells of river fish. The

remains of the hippopotamus, in particular, are so frequent, that in an area of 120 square yards, as many as six tusks of that animal were found along with various others of the bones, tusks, and horns that have been mentioned.

One horn of an ox measured as much as four feet six inches in length, and five inches in diameter at the base; and the size of this, it is observed, is the more remarkable, as another horn of an ox was found near it, only six inches long; but it is added, that they all appear to have been deposited as mere bones without the flesh; for in no instance have two bones that are connected in the living animal been found together. Although these bones have lost their cohesion, as if perished by lying long in a moist stratum, they do not seem worn in any degree, as would have happened if they had been washed by the sea for any length of time.

The third stratum in this field is sandy loam, highly calcareous, containing horns, bones, and teeth of deer and oxen, with snail-shells, and shells of river fish.

Below this stratum follow the gravel and clay corresponding to those of the other field; but as the existence of these has been ascertained solely in digging for water, it is not known, by actual examination, whether the organic remains which they may contain are of the same kinds.

On a new Construction of a Condenser and Air-pump. By the Rev. Gilbert Austin. *In a Letter to Sir* Humphry Davy, *LL.D. F.R.S.* Read March 11, 1813. [*Phil. Trans.* 1813, *p.* 138.]

Mr. Austin's object in constructing this apparatus was to impregnate fluids with any condensible gas by the aid of compression; and for the sake of preserving them in a state of purity, every part was made, as far as possible, of glass. The retort, in which the air is formed; the reservoir, in which the supply is contained; the straight tube, through which it is conveyed, and which serves as a piston; the cylinder and barrel of the pump; the receiver, containing the fluid to be impregnated; and the valves that confine it,—are all made of glass, the only exception being the stuffing of the piston, for which he names various soft materials that may be advantageously employed.

For the sake of greater simplicity in the construction, all the parts are arranged in the same vertical line. The reservoir at bottom, in which the air is first collected, is a large bell, with a perforation at the top, where it is connected with the glass rod, which serves as a piston. These are firmly fixed in position; for in this instrument, the condensation is effected by moving the barrel upon the piston, instead of having a fixed barrel with a moveable piston. Accordingly, the receiver, which is attached to the upper extremity of the glass barrel, is carried up and down with it in effecting the condensation.

The great impediment to forming pneumatic instruments of glass

appears to the author to have been the difficulty of making the joinings sufficiently firm to bear the necessary pressure, and at the same time capable of being easily disunited for the purpose of making any new arrangement of the parts; for in both these respects, as well as in other points, the usual conical joints of glass are very objectionable.

In Mr. Austin's construction, all the parts are fitted together by plane surfaces, which have the advantage, not only of being easily formed, but of fitting each other in any order of combination in which it may be thought convenient to place them. For this purpose, each part of the apparatus, where there is need of a joint, is originally made by the glass-blower to terminate in a flat, thick, circular plate of glass of a given diameter, that is larger than the neck to which it is attached.

The flat terminations of all the parts being then ground perfectly plane, any two may be applied together, and clamped by a proper system of collars and screws, with the certainty of fitting, without regard to any inequality of the perforations in each, through which the communication between the vessels is thus established.

The clamps employed by the author for connecting his apparatus, consist of two flat circular collars of brass, just large enough to admit the circular flat flanches to pass through them, and a pair of mahogany collars of smaller dimensions, so as to fit the necks after they have been sawn in two. The brass collars, having been first put upon each vessel, are prevented from returning by the interposition of the wooden collars, and are then screwed together by a suitable number of screws. In rendering these joints perfectly secure, it is necessary to turn all the screws with nearly equal force; and it may sometimes be expedient to moisten the surfaces of glass that are applied to each other when any great degree of condensation is required.

The tube through the piston terminates, at its upper extremity, in a small cup, which is the segment of a sphere, and contains a plane convex lens, ground to the same radius, and fitting so as to perform the office of a valve. A similar valve is placed between the barrel and the receiver, and another as safety-valve at the top of the receiver, with a spring and screw to regulate the force of condensation that may be applied.

Since the motion of the barrel on the piston is required to be performed with extreme steadiness, it is firmly attached by strong collars to an iron sliding-bar, guided by a dove-tail groove of brass, fixed to an upright pillar that stands on the platform, to which the first reservoir and piston are attached, so as to avoid all possibility of lateral motion that might destroy the apparatus.

The author observes, that if the undermost vessel, instead of being open as a reservoir for air to be condensed in the uppermost, be closed at the bottom, it will be exhausted, so that the same instrument serves the purpose of an air-pump as well as a condenser.

On the Formation of Fat in the Intestines of living Animals. By Sir Everard Home, *Bart. Presented by the Society for promoting the Knowledge of Animal Chemistry*. Read March 18, 1813. [*Phil. Trans*. 1813, *p*. 146.]

In the course of the author's inquiries respecting the digestive organs of different animals, he has been gradually led to suppose that the office of the colon and lower intestines in general is different from that of the upper. In the stomach and small intestines the process of forming and separating the chyle is carried on; but after the food has passed into the cæcum and colon, it appears to undergo a total change in its appearance and smell, with some tendency to putrefaction, that is not observable in the contents of the small intestines, and is prevented from being communicated to them by a valve that does not allow even gases to pass upwards into the small intestines.

The general construction also of the colon and cæca favours the opinion that the functions which they perform are of a different kind; since their capacity and arrangement would occasion the passage of their contents to be more tardy than it is through the small intestines. The smell and semi-putrescent state of these matters led to a comparison of them with animal substances buried in the ground in moist situations, which are known to be converted into adipocere, and suggested the possibility that the secondary digestive operation performed in the lower intestines might be the formation of fat; and this conjecture appeared to the author to be supported by the consideration, that fat is the winter supply in dormant animals, and that these animals have a formation of intestines peculiar to themselves, in which there is no valve to distinguish the beginning of the colon, and no other impediment to the *free* supply of materials for the production of fat.

The author next adduces an instance of the conversion of a corpse into adipocere (in the course of twenty-one years) in Shoreditch churchyard; and compares the situation of feculent matters retained in the cells of the colon with a current of more fluid matters passing over them with that of bodies buried in the neighbourhood of a common sewer; and he enumerates various instances in which substances of a fatty nature are known to be formed in the large intestines.

Ambergris, for example, is never found excepting in the last seven feet of the intestines of the spermaceti whale. In the human intestines also fatty concretions are sometimes found, called scybala, and these have a considerable resemblance to ambergris.

One instance of the formation of fatty concretions in the intestines appeared to have occurred in consequence of having swallowed large quantities of common olive oil. The consistence of these is compared to that of soft wax, and by analysis they appeared to consist of two thirds olive oil, and one third animal mucus.

A second instance is noticed, which, as well as the preceding, was observed by Dr. Babington in a child $4\frac{1}{2}$ years old, *subject to jaundice*, who has voided for some time past at intervals of ten days

or a fortnight, as much as two or three ounces at a time, of a yellow oily fluid that concretes when cold.

In consequence of such instances of fat existing in considerable quantities in the lower intestines, the author endeavoured to ascertain whether it might not be found in the common contents of the colon, and preferred those from the duck as the subject of experiment. Mr. Brande, who undertook to make this experiment for the author, divided the matter into two parts, which were kept for a week at a temperature varying from 40° to 60°; one in pure water, the other in extremely dilute nitrous acid. In the former no perceptible quantity of fat was found; but the latter yielded by this treatment about one eighth part of fat.

When a similar experiment was made on the contents of the rectum, there was no appearance of fat produced even by the action of nitric acid.

As it appeared possible that bile might perhaps assist in the conversion of animal substances into fat, the author requested Mr. Brande to try the effect of mixing muscular flesh with bile. Human muscle when digested with water alone, at the temperature of 100°, for four days, became slightly putrid without any appearance of fat; but when digested with human bile at the same temperature it became fetid on the second day, fetid and yellow on the third, and on the fourth it had the smell of excrement, was flabby, and appeared fatty on the surface.

A second experiment on beef in the bile of the ox had the same result as the preceding.

In a third experiment made also on beef with ox-bile at the temperature of 60°, there was no appearance of fat at the end of six days; and in a fourth experiment made again at 100°, there was again no appearance of grease produced by the process.

From these experiments, says the author, we learn that the bile has the power of converting animal substances into fat; that the temperature of 100°, or nearly so, is necessary for that process; and that the change is produced just as putrefaction is beginning to take place.

With a view to ascertain whether the same process could be detected actually going on in the human intestines, a quantity of fæces that had been retained upwards of six days were examined by being mixed with water at the temperature of 100°, and allowed to cool, when a film that appeared to be of an oily nature was found on the surface.

If, then, it be admitted that the origin of fat is such as is here conceived by the author, he remarks that the wasting of the body in various disorders of the lower bowels is accounted for. The uses of the various turns of the colon in different animals will be explained, and the origin of fatty concretions in the gall-bladder, which are so common, may be supposed to arise from the action of the bile on the mucus secreted from its coats.

On the colouring Matter of the black Bronchial Glands and of the black Spots of the Lungs. By George Pearson, *M.D. F.R.S.* Read February 25, 1813. [*Phil. Trans.* 1813, *p.* 159.]

It is well known that in men the bronchial glands that are situated near the bifurcation of the trachea are of a very dark colour; that in infancy the lungs in general have little or no such colour; that at the age of eighteen or twenty they have a mottled or marbled appearance, from black or dark blue spots, lines and points disseminated through them immediately under the membranous covering, and that this appearance becomes darker and darker as persons advance in age; and that the same black spots are also observable throughout the whole interior substance of the lungs when cut.

It has been conjectured by some persons that this appearance is owing to sooty matter taken in with the air during respiration, and accumulated in proportion to the duration of life; but in reply to this supposition it has been objected by others, that the same degree of blackness is not observable in brute animals, and that the theory is not supported by any accurate observations of the proportional want of intensity in those persons who might be supposed to have lived less exposed to soot by permanent residence at a distance from any large town, and from any other considerable consumption of fuel.

Dr. Pearson professes himself to be of the former opinion, and adduces a series of experiments to prove that the black matter in question is actually charcoal. In order to collect the subject of his experiments, the black glands in which it is contained are dissected out, and by washing are freed as far as possible from extraneous matter. The colouring substance is then partly pressed out, diluted with water, and found to subside from it as a black deposit. By boiling the black glands in caustic potash the whole structure is destroyed, but the black matter is not dissolved, and after a sufficient length of time subsides from the fluid. By muriatic acid also the glands are dissolved, and black matter is deposited from the solution. Nitric acid also dissolves the glands, but not the black matter which in this case floats on the surface.

By corresponding experiments on the substance of the lungs the same black deposit is obtained, but in much smaller proportion than from the glands.

The black matter thus collected and subsequently dried, having been found to yield carbonic acid by deflagration with nitre or oxymuriate of potash, Dr. Pearson considers himself warranted in concluding that it is charcoal in an uncombined state; that it is admitted along with the air in respiration; that it is retained in the minute ramifications of the air-tubes, and conveyed by the absorbents to the bronchial glands.

Experiments on the Alcohol of Sulphur, or Sulphuret of Carbon. By J. Berzelius, *M.D. F.R.S. Professor of Chemistry at Stockholm; and* Alexander Marcet, *M.D. F.R.S. one of the Physicians to Guy's Hospital.* Read May 13, 1813. [*Phil. Trans.* 1813, *p.* 171.]

The great diversity of opinions entertained by several of the most celebrated of the French chemists regarding the nature of this compound, which was originally noticed by Lampadius in the distillation of a mixture of pyrites and charcoal, induced the authors to undertake the present analysis, without any knowledge that it was again nearly at the same period under examination in France. The original opinion of Lampadius was, that it consisted of sulphur and hydrogen, and his opinion was also supported by Vauquelin, Robiquet, and the younger Berthollet. The elder Berthollet had supposed it to be a compound of sulphur, hydrogen, and carbon; but, according to Messrs. Clement and Desormes, hydrogen had appeared not to be one of its constituents, a result which is now adopted in a late report of Messrs. Berthollet, Thenard, and Vauquelin, and is here further confirmed by the inquiries of Professor Berzelius and Dr. Marcet.

Their joint paper is divided into four parts, the first of which describes the preparation and general properties of the compound; in the second, the authors examine whether hydrogen be present in it; in the third, the presence of carbon is ascertained; and in the fourth, the proportion of its constituents is determined.

The preparation consists in distilling sulphur through a red-hot tube of porcelain containing well burned charcoal, condensing the oily product in water, and subsequently rectifying it by very slow distillation at a heat between 100° and 110°, by which it is freed from a redundant quantity of sulphur which it always contains when first procured. The fluid is then perfectly transparent and colourless. It has an acrid, pungent, somewhat aromatic taste, with a smell that is nauseous and fetid. Its specific gravity is 1·272. It boils between 105° and 110°, and does not congeal at 50° below zero. It is soluble in alcohol, in ether, and in all oils whether fixed or volatile, and in alkalies; but it does not unite with water, with acids, or with any metallic substances, and even suffers no change when heated in contact with potassium.

For the purpose of determining whether hydrogen was present, the vapour of it was exploded with oxygen in the first instance. In the next, a current of oxymuriatic gas was passed through the oily liquid. Thirdly, attempts were made to burn it in oxymuriatic gas. Fourthly, the vapour of it was passed through liquefied muriate of silver; and Lastly, through various metallic oxides; but in no instance was there any appearance of water being produced, or any other evidence of the presence of hydrogen in the compound.

The presence of carbon was ascertained by the formation of carbonic acid in the combustion of the vapour with oxygen. When the oil itself was set on fire in oxygen gas, the heat was sufficiently

intense to melt a wire of platina. The presence of carbon was also proved by the slow action of barytic-water and of lime-water on the oily fluid, and consequent formation of carbonates of those earths.

When the authors endeavoured to determine the proportion in which the two constituents, carbon and sulphur, are united in this compound, considerable difficulties occurred from its volatility, and from the little affinity which it appeared to possess for the generality of chemical agents. The danger of explosion was an obstacle to the *oxidation of it by direct combustion*; and where attempts were made to analyse it by means of nitro-muriatic acid, the result proved to be a new and curious compound, that gave rise to a separate course of experiment. By means of alkaline solutions a new and unequivocal proof was obtained of the presence of carbon; but the decomposition was too imperfect for the accurate determination of proportional quantities. Recourse was therefore had to distillation of the oil through red hot oxide of iron, by which means the carbon was converted into carbonic acid, and the sulphur partly retained in the state of a sulphuret, and partly converted into sulphurous acid gas. By careful examination of these products, and by repetition of the process with corresponding results, the proportion of the constituents was found to be 84·83 sulphur and 15·17 carbon,—a result which accords extremely well with the hypothesis of two atoms of sulphur to one atom of carbon; and since the quantities obtained in this analysis corresponded with the quantity submitted to examination, this agreement tended much to confirm the opinion, that the compound does not contain any other element.

An appendix to this paper, written by Professor Berzelius, alone, contains the details of two experiments, from which the above proportions of sulphur and carbon are determined; and a statement of certain laws of determinate proportions, from which the same inference might be drawn.

The author's observation is, that when two combustible bodies unite, the quantities of oxygen which they are disposed to absorb are either equal, or one is a simple multiple of the other. In the present case the quantity of oxygen necessary to convert the sulphur into sulphurous acid is so nearly double of that which would be requisite to convert the carbon into carbonic acid, that the result obtained by supposing that to be the real proportion would not differ by one third per cent. from the proportion gained by experiment.

If Mr. Dalton's opinion be correct, that both the carbonic and sulphurous acids consist of one atom of base with two atoms of oxygen, then this sulphuret must contain two atoms of sulphur to one of carbon; but it is possible that carbonic oxide may contain two atoms of base to one of oxygen, and in that case carbonic acid must consist of one atom of each. If so, the sulphuret of carbon will accord with other sulphurets which contain one atom of sulphur to one of base. The author observes, however, that according to Sir H. Davy, there are other sulphurets consisting of two portions of sulphur to one of base; and accordingly a similar doubt occurs in

these cases, between the single or double portion of sulphur, which of them is to be regarded as the elementary atom, according to Mr. Dalton's view of the subject.

Professor Berzelius next examines various compounds, which may be termed carbo-sulphurets of the alkalies and earths. The carbo-sulphuret of ammonia sublimes unchanged in close vessels; but when exposed to air, the carbon is deposited, and hydro-sulphuret is produced; and, in the same manner, the carbo-sulphurets of lime, barytes, and strontia are decomposed when moisture is present, and hydro-sulphurets of these earths are formed.

The remainder of this appendix contains the analysis of a solid white crystalline compound, having the appearance and volatility of camphor, formed by exposure of the sulphuret of carbon during three weeks to the fumes of strong nitro-muriatic acid. It is insoluble in water, but dissolves in alcohol, ether, and in oils, whether fixed or volatile. When this compound was sublimed through a red-hot tube containing iron wire, it was decomposed, and found to consist of muriatic acid, 48·74; sulphurous acid, 29·63; and carbonic acid, 21·63.

On the Means of procuring a steady Light in Coal Mines without the danger of Explosion. By William Reid Clanny, *M.D. of Sunderland. Communicated by* William Allen, *Esq. F.R.S.* Read May 20, 1813. [*Phil. Trans.* 1813, *p.* 200.]

The author having resided several years near the coal mines in the county of Durham, has paid much attention to the circumstances of those explosions which so frequently occasion the death of many industrious people, and has contrived a lamp, which he thinks likely to answer the purpose of illumination, without any danger attending its use.

He is of opinion, that ventilation, as at present practised, has little or no effect in preventing explosions; since it has no tendency to diminish the quantity of inflammable gas emitted by the old workings, which must always be in danger of exploding wherever it comes into contact with atmospheric air, if light be applied to it. The partitions and folding doors put up at the entrances of old workings appear to be very inadequate to prevent the occurrence of such explosive mixtures; and their frequency is shown by the number of accidents which the author enumerates as having taken place in his own neighbourhood alone in the course of the last seven years. The number of explosions in the course of that time has been six; and these have destroyed more than two hundred pit-men, who have left wives and children in a state of poverty and distress. In some instances, large pumps have been erected at the top of the shaft, worked by steam-engines, for the purpose of drawing off the inflammable gas from those parts where it most abounds; but even these have been found insufficient, since the engine will not, in all instances, be applied to the part where it is most wanted: and it is estimated, that wherever

the quantity of inflammable gas amounts to one twelfth part of the atmospheric air present, an explosion may take place.

For the purpose of preventing such accidents, Dr. Clanny has contrived to insulate a candle, by water placed both above and below the lantern in which it is contained. The air, which is intended to support the flame, is supplied by means of a pair of common bellows, by which it is forced through the water beneath the flame; and it is again emitted, after having supported the combustion, by a bent tube that passes into water from the top of the lantern.

In consequence of this arrangement, if the air of the mine becomes liable to inflame, the explosion will be confined to the mere content of the lantern, of which only a small part will be consumed, unless the quantity of inflammable gas be very suddenly increased.

This communication is accompanied by drawings of the lantern and its parts in detail, whereby any workman may be enabled to execute it according to the design of the author.

On the Light of the Cassegrainian Telescope, compared with that of the Gregorian. By Captain Henry Kater, *Brigade-Major. Communicated by the Right Hon. Sir* Joseph Banks, *Bart. K.B. P.R.S.* Read May 27, 1813. [*Phil. Trans.* 1813, *p.* 206.]

The author having remarked the performance of a Cassegrainian telescope, made by a self-taught artist at Ipswich, to be superior to what he believes is usually expected from telescopes of this construction, has been led to make a series of experiments on the comparative illumination given by the Cassegrainian compared with that obtained by the Gregorian construction. For though the Cassegrainian form has been considered merely as the Gregorian disguised, and has been rarely adopted, in consequence of its inverting objects, a superior power of illumination, if correctly ascertained to exist, may prove a valuable property, in addition to its advantage of being considerably shorter than the Gregorian.

In the telescope first compared by Major Kater, the specula were cast at the same time, in the same metal, and to the same pattern. The magnifying powers of the two instruments were ascertained by experiment to be very nearly equal, but with a small excess on the side of the Cassegrainian. The two telescopes were placed side by side, and pointed to the same object, which was a printed card, at the distance of fifty yards; and as the brightness, as seen in the Cassegrainian, was far superior, its aperture was first reduced by a ring of pasteboard, and then gradually enlarged till the card appeared equally bright through both telescopes. After the respective areas of aperture in each telescope had been measured, with due allowance for the light obstructed in each by the small mirror, that of the Cassegrainian was found to be to the Gregorian as 46 to 108, or 3 to 7 nearly.

In the second comparison made by Mr. Kater, the Cassegrainian

telescope had been made some time before the Gregorian, and its speculum had in consequence lost somewhat of its original polish. But notwithstanding this source of disadvantage on the side of the Cassegrainian, a corresponding superiority again appeared in its power of illumination.

The areas of aperture were in this instance as 79 to 110; but as the magnifying powers were not equal but in the ratio of 108 to 182, it was necessary to make further allowance, in proportion to the squares of these numbers; so that the illuminating powers were found to be nearly as 3 to 2.

From the mean of these experiments, and from consideration of all circumstances, the author conceives that the relative superiority of the Cassegrainian may be stated to be as 60 to 33, or 20 to 11.

With respect to the probable cause of the difference thus observed, Major Kater conjectures that it may possibly depend on the mutual interference of rays meeting in the same point, which it is possible may be in great measure dissipated when received by the small speculum in the Gregorian, after crossing in the principal focus; while on the contrary, in the Cassegrainian, the loss of light from this source is avoided, since the small speculum in that construction receives the rays before they arrive at the focus, and before they become sufficiently concentrated to interfere with each other's motion.

This conjecture, it is observed, derives additional support from a circumstance that has been observed with respect to refracting telescopes; namely, that in a comparison between the simple astronomical telescope and a Galilean of equal aperture and power, the satellites and belts of Jupiter may be seen much more distinctly in the latter, where the rays are received by a concave lens before their intersection in the principal focus of the object-glass.

Additional Observations on the Effects of Magnesia in preventing an increased Formation of Uric Acid; with Remarks on the Influence of Acids upon the Composition of the Urine. By William Thomas Brande, *Esq. F.R.S. Prof. Chem. R.I. Communicated by the Society for improving Animal Chemistry.* Read June 3, 1813. [*Phil. Trans.* 1813, *p.* 213.]

This communication consists of two parts: the first of which is a confirmation of the beneficial effects of magnesia in preventing the deposit of uric acid from the urine of persons subject to the formation of a redundancy of that ingredient; and the second part relates to the trial of acid remedies in disorders of an opposite nature, where the urine is found to deposit either the ammoniacal phosphate of magnesia or phosphate of lime.

The first section contains two cases; the first of a gentleman who was accidentally induced to employ magnesia for the purpose of relieving indigestion, occasioned by the use of alkaline remedies, and who thereby fortunately corrected a tendency to form red sand, for which those medicines had been taken ineffectually.

The second is an instance of the beneficial effects of magnesia in correcting the formation of uric acid, where alkalies had relieved but could not be continued. It also exhibits the prejudicial effects of both when too long persisted in, by causing a redundance of an opposite nature in the urine, and a consequent deposit of earthy phosphates as white sand.

The object of the second section of this paper is to ascertain what acids may be employed with most advantage in those cases where the ammoniacal phosphate of magnesia prevails in the urine, either naturally or by an injudicious use of alkaline medicines. For though the use of acids was pointed out fifteen years since by the same chemist who originally analysed this species of calculus, Mr. Brande is not aware that in the course of that time any experiments have been made to illustrate the mode of action of different acids.

The first case related is that of a gentleman who had been cut for the stone ten years before, and was again attacked with symptoms of calculus. By the use of a brisk purge in the first instance, one calculus was passed, but he still suffered pain in his kidneys, that was aggravated by the use of soda water, which increased the proportion of triple phosphate in his urine. Ten drops of muriatic acid were then taken three times a day, which removed the white sand; but as this acid tended to increase the quantity of red sand, it was exchanged for carbonic acid, with decided relief to all the symptoms.

The second case was that of a boy, from whom four calculi had been extracted by the usual operation of lithotomy. These consisted principally of the triple phosphate of magnesia; and his urine continued after the operation to be loaded with a deposit of this substance, in the form of a white sand. The acid employed in this instance was citric acid, to the amount of twenty grains three times a day. As the effect of this remedy upon the disorder was found to be decisive, and the consequence of any omission was observable in less than twenty-four hours, it was persevered in for nearly three months, when the disposition to deposit the earthy phosphates was found to have ceased altogether, and the lad remained apparently free from disease.

Two other cases are also related: the first of which was relieved by citric acid taken twice every day, together with vinegar and other acid articles of diet; and the second by carbonic acid, taken as an effervescing draught, which prevented the formation of white sand, and removed an irritation of the bladder, occasioned by that deposit.

As the general results of these trials, the author infers,—

That where alkalies fail to correct the redundant secretion of red sand, magnesia is generally effectual.

That when either of these is improperly continued, they are liable to occasion the deposit of the earthy phosphates, in the form of white sand.

That the mineral acids prevent this deposition, but are apt to induce a return of red gravel, consisting of uric acid.

That vegetable acids, especially the citric and tartaric, are less

liable to produce this prejudicial effect, although they prevent the formation of white sand;—

And that carbonic acid appears particularly useful in cases where the irritability of the bladder forbids the use of other acid remedies.

Additions to an Account of the Anatomy of the Squalus maximus, *contained in a former Paper; with Observations on the Structure of the Bronchial Artery, By Sir* Everard Home, *Bart. F.R.S.* Read June 24, 1813. [*Phil. Trans.* 1813, *p.* 227.]

The author observes, that with respect to his former description of the external parts, he has no addition to make, excepting that, from the position of the fish at the time when it was drawn, a small fin was omitted between the anus and tail; which, however, is so far important, that some persons have on this ground imagined that the fish described was a different species of Squalus.

Of the internal parts, the stomach is now described, and a delineation given. The liver has six ducts; but these unite, before they enter the duodenum, in one common cavity, which corresponds to the dilatation of the common duct in those quadrupeds that have no gall-bladder.

A drawing is given of the heart and valves of the bronchial artery: and it is remarked, that the coats of this artery are muscular to some distance from the ventricle, the use of which is conceived by the author to be to aid in propelling the blood through the gills when impeded by the pressure of any great depth of water: for the same quantity of blood should pass through the gills at all depths, unless the water at greater depths contained a larger proportion of air; but this did not appear to be the case by direct experiment, in which the author was assisted by Mr. Brande.

In support of the opinion that muscularity of the coats of the bronchial artery is connected with the circumstance of living at great depths, the author remarks, that in the turbot and the wolf-fish, which live in shallow water, there is no muscular covering to the origin of the bronchial artery, but, on the contrary, its coats are so exceedingly elastic as to be easily dilated into a considerable reservoir.

In the *Lophius piscatorius*, the mechanism of the heart is such, that description alone could hardly convey an adequate idea of its form; and a drawing of its peculiarities is annexed.

In the tribe of Mollusca, other differences of muscularity occur from other causes. In the Teredines, where great muscular power is required for working the boring engine, the heart consists of two auricles and two ventricles, with valves of very great strength, to direct the current of the blood. But in the oyster, on the contrary, the heart has but one auricle and one ventricle; and in the muscle, the heart is scarcely divided into auricle and ventricle, but is a mere oval bag, through which the intestine passes, the peristaltic motion of which appears to the author to contribute as much to the motion of the blood as can be effected by the proper coats of the ventricle.

After this digression concerning varieties in the structure of the heart, the author resumes his anatomy of the *Squalus maximus*, and notices, among the peculiarities of the urinary passages, that there is no proper urinary bladder, but a cavity comparatively small, that is common to the urine and semen; since both the vasa differentia and ureters open into it at the distance of about four inches from each other.

The holders in the male Squalus have been noticed before; but it is now added, that in each holder is a canal, communicating with a cavity between the skin and muscles of the abdomen, which is stated to be eleven feet long and two wide. The surfaces of this cavity are white, and extremely smooth; and it contains a mucus more viscid than any known animal secretion.

Respecting the brain, it is remarked, that in the Squalus, as well as in fish in general, the cerebrum is wanting, unless the part from which the olfactory nerves arise be so called.

The eye is very small for the size of the fish; its largest diameter being not more than three inches, and its shortest only one inch and three quarters.

The straight muscles that surround the eye are so much stronger than would appear to the author requisite for moving so small a sphere, that he thinks such strength can only be required in adjustment of the eye for overcoming the stiffness of the sclerotic coat.

Some further Observations on a new detonating Substance. In a Letter from Sir Humphry Davy, *LL.D. F.R.S. V.P.R.I. to the Right Hon. Sir* Joseph Banks, *Bart. K.B. P.R.S.* Read July 1, 1813. [*Phil. Trans.* 1813, *p.* 242.]

In a second letter which the author received from France, he is informed that the detonating oil was there originally procured by passing a mixture of chlorine and azote through aqueous solutions of sulphate or muriate of ammonia; but from the method of obtaining it in this country, it is evident that the azote is unnecessary, as it is obtained by exposure of any ammoniacal salt in solution to mere chlorine. The oily fluid obtained by these means has the specific gravity of 1·653. It is not congealed by cold, as the author had formerly supposed, but remains fluid even when cooled by a mixture of ice and muriate of lime.

When kept in water, it gradually disappears, and the water becomes acid, having the taste and smell of weak nitro-muriatic acid.

Concentrated muriatic acid decomposes it, disengaging chlorine, and forming muriate of ammonia.

In concentrated nitric acid it gives out azote.

In dilute sulphuric acid it yields a mixture of azote and oxygen.

In a strong solution of ammonia it detonates; with a weak solution it yields azote.

With the muriates of sulphur and of phosphorus, or with sulphuret of carbon, it combines without any violence.

When it is kept in contact with mercury alone, it yields azote and a white powder, consisting of a mixture of calomel and corrosive sublimate.

One means of estimating the proportion of the elements was obtained from this mode of analysis, and it appeared to be 19 azote to 81 chlorine.

In such attempts as were made to decompose this substance in exhausted vessels, the tendency to explosion was such, that no estimate could be formed of its elements, from the small quantities on which it was safe to operate.

The mode of analysis on which the author places the most reliance, is that performed by means of muriatic acid. According to his view of the play of affinities in this process, ammonia is formed by the union of the azote in the compound with the hydrogen of one part of the muriatic acid, occasioning the chlorine of both to be set free; while the ammonia so formed combines with another portion of the muriatic acid, and is found in the solution as muriate of ammonia. In addition to the quantity of chlorine actually evolved in this mode of trial, it was necessary to estimate the quantity remaining dissolved in the liquid. For this purpose the sulphuric solution of indigo was employed, and the quantity of chlorine estimated by the quantity of blue colour destroyed.

From the results of two experiments, the author infers that nine grains of azote are combined with 91 of chlorine; and since this proportion accords very nearly with the supposition of one volume of azote with four equal volumes of chlorine, he regards the present as a satisfactory instance of the law of definite proportions; for the estimate obtained by the action of mercury upon the oil, differs no more than might be expected from the nature of the experiment.

Since one of azote combines with three of hydrogen to form ammonia, and three of hydrogen combine with three of chlorine in muriatic acid, the author had thought it probable that one of azote would have been found combined with three of chlorine, but is now of opinion that no strict laws of analogy are to be found from which we can form a previous judgement of such combinations; and he takes occasion to remark, that other philosophers who have presumed that azote contains oxygen, are not warranted in their inference by any laws that he has observed.

Experiments on the Production of Cold by the Evaporation of the Sulphuret of Carbon. By Alexander Marcet, *M.D. F.R.S. one of the Physicians to Guy's Hospital.* Read July 8, 1813. [*Phil. Trans.* 1813, *p.* 252.]

In a former paper which the author communicated jointly with Professor Berzelius on sulphuret of carbon, its remarkable volatility was noticed; and as it appeared likely on that account to produce a great degree of cold by evaporation, Dr. Marcet has been induced to make a course of experiments on that subject.

When the bulb of a thermometer, wrapped in fine lint, has been dipped in the sulphureous liquor, if it be simply exposed to the air it sinks to about zero of Fahrenheit's scale, although by a similar evaporation of ether the cold produced is not below 20°.

If a thermometer, coated as before, and wetted with the sulphuret, be placed in the receiver of an air-pump, a cold of 65° or 70° below 0° is easily obtained, by a vacuum which supports one fourth of an inch of mercury; and if the air-pump can exhaust as far as one eighth of an inch, the thermometer sinks to −81° or −82° in less than two minutes, even though the thermometer at the commencement of the experiment was as high as 70° above 0.

Hence the freezing of mercury is an experiment that may be performed at any time, and with no more apparatus than a common air-pump, and enough of the sulphuret to moisten the bulb of a thermometer. Since sulphuric acid has no affinity for the sulphuret, it has no effect in adding to the degree of cold produced upon the principle upon which that is employed by Mr. Leslie, excepting in so far as it removes any moisture that may be present in the air, and which in some measure impedes the process until it is converted into a hoar frost, that may be seen adherent to the bulb of the thermometer.

On a saline Substance from Mount Vesuvius. By James Smithson, *Esq. F.R.S.* Read July 8, 1813. [*Phil. Trans.* 1813, *p.* 256.]

From the strong evidence we have that a very large proportion of the world, as we now see it, has at some period been either in a state of actual combustion, or has felt the effects of heat, a high interest, says the author, attaches itself to volcanoes and their ejections, as partial instances of similar operations now going on.

In support of the igneous origin of primitive strata, it is observed, not only that no *crystal* imbedded in them contains water, but that none of the *materials* of the strata contain water in any state.

The subject of the present experiments was thrown out in a liquid state from the cone of Vesuvius about the year 1792 or 1793.

It was of a dirty white colour, with streaks of yellow and green.

When heated, it fused without any loss of weight. When fused on charcoal, it was converted into sulphuret of potash.

In water it dissolved readily, leaving particles of specular iron and oxide of copper. Muriate of platina caused a copious precipitate from the solution, from the presence of potash. Nitrate of barytes afforded an abundant precipitate of sulphate of barytes.

Sulphate of silver gave a curd-like precipitate, showing the presence of muriatic acid.

Prussiate of soda gave a red precipitate, consisting of prussiate of copper. Carbonate, or oxalate of soda or potash, occasioned no precipitation of any kind of earth; nor did any means employed detect the presence of boracic, or of any other acid, excepting the sulphuric and muriatic.

By a suitable course of experiment, the proportions of the several salts present were found to be nearly thus:

Sulphate of potash....................	71
Sulphate of soda........................	19
Muriate of soda	5

the remainder consisting of a little muriate of ammonia, mixed with the muriates of iron and copper.

In the part which remained undissolved by water, there was also found to be submuriate of copper, similar in composition to the green sand of Peru, and a yellow powder that was judged to be submuriate of iron; so that, on the whole, this single mass presented as many as nine different species of matter.

Some Experiments and Observations on the Substances produced in different chemical Processes on Fluor Spar. By Sir Humphry Davy, *LL.D. F.R.S. V.P.R.I.* Read July 8, 1813. [*Phil. Trans.* 1813, *p.* 263.]

In the Bakerian lecture for 1808, the author had supposed fluor acid to be decomposed when potassium is heated in silicated fluoric acid gas, and that oxygen was separated from it; an inference which had also been drawn from a similar experiment by Messrs. Gay-Lussac and Thenard. But when he afterwards found that oxymuriatic acid could not be decomposed, and that no oxygen could be separated from the compounds of this body with phosphorus, sulphur, or the metals, he was led to conceive an analogy between the oxymuriatic and fluoric compounds, an analogy also suggested to him by M. Ampere.

The experiments described in the present paper are principally guided by this analogy, with a view to ascertain whether it is well founded.

The subjects of experiment are silicated fluoric gas, originally discovered by Scheele. Liquid fluoric acid in its concentrated state, first obtained by Messrs. Gay-Lussac and Thenard, and the fluo-boric acid of the same chemists.

When these compounds are acted upon by potassium or sodium, the results are fluates of potash or soda, with silicum, hydrogen, or boron, according to the compound operated upon.

With regard to these results, there are three hypotheses which may be maintained. One is, that fluoric acid consists of an inflammable base united to oxygen. A second, that it consists of a simple base, which may be called fluorine (analogous to chlorine), united with hydrogen. A third is, according to the tenets of the phlogistians, that fluoric acid, like metallic oxides, is liable to combine with hydrogen, and form an inflammable compound. Since all the phenomena may be explained according to any one or other of these hypotheses, the sole question is, which of these explanations is best, as being most conformable to the general series of chemical facts with which we are at present acquainted.

Sulphuric and nitric acids which, according to the first supposition, are inflammable bases combined with oxygen and water, when acted upon by ammonia yield water, but fluoric acid in combining with ammonia gives out no water.

Sulphate of ammonia, by the action of potassium, yields sulphur and ammonia; and in the same manner nitrate of ammonia yields its base azote, with ammonia. But when fluate of ammonia is acted upon by potassium, the only product beside ammonia is hydrogen, just as in the action of potassium upon muriate of ammonia, which yields only hydrogen and ammonia. In the latter case chlorine combines with the potassium; and it would appear that a similar base is contained in the fluate of ammonia.

By the voltaic battery also, hydrates of such bodies as are known to contain oxygen, as sulphuric acid, hydrophosphorous acid, and nitric acid, all yield oxygen at the positive wire, and at the negative they give out their base, or a suboxide, along with hydrogen.

The strong action of fluoric acid on almost all bodies, occasioned considerable difficulty in attempting to collect the products of its electrization. But in a tube of horn silver, when it was electrified by a wire of platina at the positive pole, the wire was covered with a chocolate-coloured powder, but no oxygen was extricated.

When a piece of plumbago was placed as the positive conductor in fluoric acid, it was quickly destroyed, and a subfluate of iron was deposited at the negative surface, the fluid becoming turbid and black.

These and other phenomena of electrization appear to the author not favourable to the supposition of fluoric acid consisting of an inflammable base combined with oxygen; but to be best explained by supposing it to be, like muriatic acid, composed of hydrogen, which appears at the negative pole, and a peculiar principle to be termed fluorine, which, like chlorine, is negative, and is attracted by the positive pole, and in general combines with the metal, which is there exposed to its action.

If, then, according to this supposition, we assume that fluates are combustibles united with fluorine, this principle cannot be obtained separate by means of any other combustible, as these will only form new compounds with it; but we may hope to effect the separation by means of oxygen or chlorine, as these in certain cases separate each other. And since the fluates of silver, mercury, and potash, are decomposed by *muriatic acid*, the author exposed these compounds also to the action of chlorine, in the hope that fluorine might by that means be disengaged.

But though these fluates were each decomposed, the matter separated from them acted upon the vessels containing them with too much energy to be exhibited in a separate state.

There seems, however, says the author, great reason to suppose that a particular principle *is* separated from them analogous to chlorine, and that when it can be obtained separate, it will be found to be a gaseous body.

He estimates the number which should represent fluorine at less

than half that of chlorine, and so little exceeding that of oxygen, that those who would suppose it to contain oxygen combined with an inflammable base, must suppose the base to be less than one twentieth part of the oxygen with which it combines.

Catalogue of North Polar Distances of Eighty-four principal fixed Stars, deduced from Observations made with the Mural Circle at the Royal Observatory. By John Pond, *Esq. Astronomer Royal, F.R.S.* Read July 8, 1813. [*Phil. Trans.* 1813, *p.* 280.]

A Synoptic Scale of Chemical Equivalents. By William Hyde Wollaston, *M.D. Sec. R.S.* Read November 4, 1813. [*Phil. Trans.* 1814, *p.* 1.]

The design of the scale here proposed by the author is to save chemists the labour of many troublesome computations in estimating the ingredients of neutral salts, and the reagents and precipitates by which these ingredients might be ascertained.

For though certain laws to which chemical union is subjected have of late been discovered, and have enabled chemists to determine with greater precision than formerly the composition of bodies submitted to be examined, and to express numerically the relation of the several elementary chemical substances to each other; nevertheless the computations requisite for applying these results to many objects of inquiry are frequently attended with considerable trouble.

The author briefly sketches the history of proportional chemistry, beginning with Bergman, who, perceiving that the same acid united to the same base, always in the same proportion, took pains to ascertain the composition of various salts. Kirwan followed the same line of endeavour to a greater extent, with a view to determine the proportions of various acids to different bases, as questions independent of each other. To these succeeded Richter, who gave connection to the subject by observing a new relation that had escaped the notice of Bergman, Kirwan, or any of his predecessors. They had observed only the constancy of the proportion of the same acid to the same base; Richter observed, further, a fixed relation of acid to acid: namely, that when the proportional quantities of any two acids, that are each sufficient to saturate a given quantity of any one base is determined, the same proportional weights of these acids will also saturate equal quantities of any other base; and consequently that if any quantity of sulphuric acid be assumed as standard, then equivalent quantities of all other acids may be conveniently expressed by fixed numbers, adapted to each; and the several quantities of different alkalies and earths that would each saturate the standard quantity, might also be represented constantly by corresponding numbers.

The observation of other proportions, which are simple multiples of the preceding, by Mr. Dalton and others, are noticed as affording an important correction of the best analyses; but it is observed that

the theory of atoms by which these facts are explained is by no means of importance to the present inquiry. It is by means of a series of numbers computed according to the method of Richter, that this scale is constructed so as to answer at one view the very numerous questions that occur to an analytic chemist in the examination of any saline compound. It is similar in principle to the common sliding rule, and like that instrument has the usual Gunter's line of numbers on the slider; but upon a line adjacent to the slider are marked certain points corresponding to those numbers which represent the various chemical elements, acids, alkalies, and other compounds intended to be included in the present view. By motion of the slider any one point of the line of numbers, as 100, may be made to correspond with the point indicating any compound, as sulphate of potash. By the position of the point for sulphuric acid, this salt is seen to contain 46 of acid, and the other ingredient potash at the same time corresponds with 54 on the slider. By the position of the point for sulphate of barytes, it appears that 135 of this precipitate would be obtained from 100 of the salt, and in the same manner that it would yield 176 of sulphate of lead, with a great variety of similar answers respecting the equivalent quantities of other compounds in which the same quantities of acid or neutralizing base is contained.

Since the line of numbers is so divided that a given space of every part of it corresponds to numbers that bear a given ratio to each other, and since the intervals on the adjacent column of equivalents are all laid down according to certain given portions of the same scale, they directly indicate by juxtaposition numbers that are in the same proportion on any part of the scale that may be presented to them, as will be very evident to those who are acquainted with the common properties of other sliding rules.

For the sake of those who may not be accustomed to the use of the sliding rule, and for the purpose of recommending that valuable instrument to more general use, the author enters rather more than might otherwise be requisite into the elementary principles of logometric division.

Methods of clearing Equations of quadratic, cubic, quadrato-cubic, and higher Surds. By William Allman, *M.D. Communicated by the Right Hon. Sir* Joseph Banks, *K.B. P.R.S.* Read July 8, 1813. [*Phil. Trans.* 1814, *p.* 23.]

In a paper communicated to the Royal Irish Academy by Dr. Mooney, the method of exterminating any number of quadratic surds is pointed out by successively squaring them when brought alone to one side of the equation; and the present is an extension of the same method: first, to all surds whose indices are any integral power of 2, as the fourth, eighth, sixteenth, thirty-second power, &c.; and next to cubic surds, and to any number of surds whose common indices are in any manner compounded of the factors 2 and 3; next

to any combinations of surds whose indices do not exceed the number 6, and to as many as *three* surds, neither of whose indices exceed 12, as well as to various others which cannot be concisely specified.

Analysis of a new Species of Copper Ore. By Thomas Thomson, *M.D. F.R.S. L. and E.* Read November 18, 1813. [*Phil. Trans.* 1814, *p.* 45.]

The mineral here analysed was brought from the peninsula of Hindostan by Dr. Heyné, where it occurs in considerable quantity along with malachite. Those specimens that are freest from malachite are of a dark blackish brown colour, soft, being easily scratched with a knife, which leaves a streak of a reddish brown. Its specific gravity is 2·62. Its fracture is in general small-conchoidal, but with a tendency in some parts to a foliated fracture; but it has not yet been seen with any appearance of external crystalline form.

It effervesces with acids, which form a blue or green solution according to the acid used, and leaves a red powder undissolved.

One hundred grains treated with dilute sulphuric acid lost 16·7 grains by escape of carbonic acid gas.

One hundred grains having been treated with muriatic acid formed a green solution, from which a clean plate of zinc precipitated 48·5 grains.

The red powder left by cold muriatic acid was digested for several hours in nitro-muriatic acid, which left 2·1 grains of white quartz undissolved, and afforded by ammonia a precipitate of 19·5 grains oxide of iron.

In order to determine the state of the oxide of copper in this ore, Dr. Thomson put 100 grains in fine powder into the bottom of a tall narrow vessel, which he then filled with water, and by means of a funnel poured a quantity of muriatic acid on the ore at the bottom. Since the ore was even in this mode immediately attacked, and formed a solution which from the commencement appeared green, he considers this evidence decisive, that the copper is in the state of black oxide, in which 100 of the metal are combined with 25 oxygen; so that 48·5 of copper precipitated by zinc indicated 60·75 of black oxide in the ore, and the analysis thus conducted gives an amount of ingredients corresponding within one per cent. with the quantity originally taken for experiment.

Since the integrant parts of carbonic acid and of oxide of copper, as the author has elsewhere shown, are to each other in the ratio of 2·75 to 10, and as this is just the ratio of 16·7 to 60·75, the quantities contained in the ore, there can be no doubt that the carbonic acid and copper are combined in the ore, constituting a carbonate of copper without water, and in that respect differing from both malachite and the blue carbonate, the former of which would appear from Klaproth's analysis to contain two particles of water, and the latter one. So that the present ore may be distinguished by the name of anhydrous carbonate of copper.

The Bakerian *Lecture: on some new Electro-chemical Phenomena. By* William Thomas Brande, *Esq. F.R.S. Prof. Chem. R.I.* Read November 25, 1813. [*Phil. Trans.* 1814, *p.* 51.]

When any decompositions are effected by means of the voltaic battery, it is known that one of the constituents is attracted towards the positive pole, and the other to the negative. Of the ultimate chemical elements a very small proportion is attracted by the former, by far the greatest part being attracted by the negative pole; and it is thence inferred that these are themselves possessed of inherent positive electricity.

Although all the differences observable between voltaic and common electricity have already been shown to depend solely upon difference of quantity and of intensity, Mr. Brande has thought it would be desirable to trace their relation, with regard to a series of phenomena that have not yet undergone a comparative examination.

When the flame of a candle is interposed between two bodies oppositely electrified, Mr. Cuthbertson observed that heat passes to the negative surface, and thence inferred the passage of the electric fluid in the same direction. It occurred to Mr. Brande, that possibly this effect might depend on the nature of the substance employed as the combustible body; and by substituting the flame of phosphorus instead of the candle, he found the effects were reversed: for then the positive surface became considerably warmer than the negative, the flame itself being now visibly attracted towards the positive ball, instead of inclining like the flame of the candle toward the negative.

The rapid formation of acid matter during the combustion of phosphorus, appeared sufficient to occasion the attraction of this flame to the positive side, in conformity to what occurs in voltaic experiments; and Mr. Brande conceives the carbon and hydrogen which abound in the flame of the candle to be the cause why that flame takes the opposite direction.

When the flame of olefiant gas was substituted, the negative ball was 10° warmer than the positive; the flame of sulphuretted hydrogen gave only 3° excess to the negative ball; that of phosphuretted hydrogen communicated 2° to the positive ball. Arseniuretted hydrogen heated the negative ball most, though the fumes of white arsenic were visibly drawn toward the positive ball. With flames of hydrogen or of carbonic oxide the difference of temperature was too small to be fully depended on, but the flame of the latter was directed towards the positive ball. With respect to sulphur, no particular direction was observed to be given to the flame, but the vapour passed toward the positive ball. When potassium in a state of combustion was placed between the electrified balls, both the flame and the fumes were drawn to the negative conductor. The attraction of other fumes being tried, not in the state of combustion, ammonia afforded no distinct result. Muriatic gas was visibly attracted by the positive pole; and nitrous acid gas was also drawn in the same direction. The fumes that arise from benzoin and from

amber, when moderately heated, were attracted as acids to the positive side; but when these bodies were ignited so as to admit a dense smoky flame, the carbonaceous matter was drawn, like that of other resinous bodies and camphor, to the negative ball.

The majority of these instances, says the author, serve well to illustrate the inherent electrical states of different species of matter, and give a further proof of the identity of common and voltaic electricity, and especially the attraction of the fumes of the phosphoric and benzoic acids to one side, and of the fumes produced by the combustion of potassium and camphor to the other.

But there are some phenomena that did not turn out as might have been expected. The combustion of carburetted hydrogen, for instance, gives rise to the production of water and of carbonic acid; but its flame is attracted by the negative surface. It is conceived, however, that this direction may be given to the flame by its carbonaceous contents rather than by the products of its combustion.

Mr. Brande is of opinion that these experiments may suggest a fair explanation of the phenomena presented by those bodies that are termed unipolar by Mr. Erman, because when connected with one or other extremity of the voltaic battery, they transmit the influence of only one species of electricity. The flame, for instance, of oil or of wax must be considered as consisting chiefly of these bodies in a state of vapour; and as their natural electricity is positive, they will have no tendency to destroy that of a positive pole with which they are connected, and the gold leaves of an electrometer will continue to diverge; but when they are applied to a negative pole, their inherent positive electricity will neutralize that of the battery to which they are united, and the gold leaves will in consequence collapse.

An Account of some new Experiments on the fluoric Compounds; with some Observations on other Objects of Chemical Inquiry. By Sir H. Davy, *LL.D. F.R.S. V.P.R.I.* Read February 13, 1814. [*Phil. Trans.* 1814, *p.* 62.]

Since the date of those attempts of the author to decompose fluorine, of which an account has already been printed in our Transactions, and from which he inferred that pure liquid fluoric acid consisted of hydrogen united to a base which he had not then been able to procure in a separate form, but which is detached from the hydrogen by various metals, he has made many experiments that in his opinion tend to confirm this idea, though all his attempts to effect the actual decomposition have been unsuccessful.

Fluate of lead, which, according to the author's view of its constitution, consists of lead united to the peculiar fluoric principle, is not decomposed by dry ammonia; but by liquid ammonia it yields oxide of lead, in consequence of the decomposition of water which gives oxygen to the lead, and hydrogen to the acid which now enters into the composition of fluate of ammonia.

So also silicated fluate of ammonia, or fluoborate of ammonia, when acted upon by chlorine, yield no silica, or boracic acid, unless in consequence of the presence of moisture; but they form muriate of ammonia, and either silicated fluoric or fluoboric gas.

When charcoal was ignited in either of these gases no decomposition was effected, but only a disengagement of a little inflammable gas from the charcoal. Neither was liquid fluoric acid decomposed by charcoal heated to whiteness in a tube of platina.

According to the author's experiments on the decomposition of fluor spar by sulphuric acid, the sulphate of lime which remains after complete decomposition weighs more than the spar decomposed in the proportion of 175 to 100. But in order to obtain this result, it is necessary to collect the very purest white Derbyshire fluor, and to distil repeatedly to dryness, after the addition of fresh acid at each repetition. By computing upon the eighth result, and supposing the number representing calcium to be 40, that for fluorine is estimated to be 34·2.

By forming fluate of potash from a known quantity of subcarbonate, the number obtained for fluorine appeared to be about 32·6.

From these experiments, and others made on the quantity of fluate of potash obtained from hydrate of potash, the author infers that the number representing fluorine may be estimated at about 33.

Two cubical inches of ammoniacal gas, weighing 36 grains, were found to combine with one of silicated fluoric gas, which were found to weigh 110·7, the number for which is thence inferred to be 98·4; and it is presumed to consist of two parts fluorine, and one of silicious basis.

The author has made many experiments with the hope of determining the quantity of oxygen in silica, but has not succeeded to his satisfaction. However, since one part of silica requires more than three times its weight of potassium to decompose it, this seems to show that silica cannot contain much less than half its weight of oxygen. But he has not been able to obtain its basis in a separate state so as to ascertain its exact nature.

Sir Humphry Davy has at various times made many experiments to endeavour to detect oxygen in chlorine, in conformity to the opinion still maintained by many persons, that this is one of its elements, but without success. Sulphuret of lead when acted upon by chlorine, gave the muriates of sulphur and of lead, and not sulphate of lead, as might possibly be expected. Neither is muriate of lead decomposed by sulphureous acid gas, which might be expected to take oxygen if any were present.

It appears, on the whole, to the author impossible to give stronger evidence of chlorine being undecompounded. In his estimation it ranks with gold, silver, hydrogen, or oxygen. He admits that persons may doubt whether these are elements, but thinks it not philosophical to doubt whether it has yet been decompounded.

In reply to some arguments lately advanced by Professor Berzelius in favour of the presence of oxygen in chlorine, deduced from the

laws of multiple proportions, Sir Humphry Davy observes, that the fact is, that the oxygen which Professor Berzelius supposes to be in the chlorine is combined with the metals; and that with respect to any regularities among multiple proportions, there is no general law observable. Azote, for instance, combines with three volumes of hydrogen. When combined with oxygen it may be united to $\frac{1}{2}$, 1, 2, or $1\frac{1}{2}$ of the same body, and in combination with chlorine it unites with four volumes.

The author combats the notion of oxygen being considered as the principle of acidity, and contends that hydrogen enters into the composition of nearly as many acids as oxygen; that chlorine and fluorine are merely bodies of the same class, which like oxygen combine with great energy, but do not owe these properties to the presence of any oxygen contained in them.

Some Experiments and Observations on a new Substance which becomes a violet-coloured Gas by Heat. By Sir Humphry Davy, *Knt. LL.D. F.R.S.* Read January 20, 1814. [*Phil. Trans.* 1814, *p.* 74.]

The discovery now announced to the Society was made about two years since by M. Courtois, a manufacturer of saltpetre at Paris. It is procured from the ashes of sea-weeds: after the extraction of the carbonate of soda, the addition of strong sulphuric acid extricates this substance in the form of a violet vapour, which condenses in crystals, that have the colour and lustre of plumbago. The colour of its vapour has occasioned the French chemists to give it the name of iode, from ἰώδης, *violaceous*.

Specimens of this substance were given to MM. Desormes and Clement, who have given a memoir upon it to the Imperial Institute, describing its principal properties. Its specific gravity is said to be about 4. It volatilizes at a temperature rather below that of boiling water. It combines with phosphorus, with sulphur, with metals, metallic oxides, and with alkalies, forming with ammonia a detonating compound. It dissolves in alcohol, or ether; and with hydrogen it forms a compound very similar to muriatic acid gas, but which M. Gay-Lussac, in a memoir read to the Institute, shows to be a peculiar acid, distinct from the muriatic: and he compares the body itself to oxymuriatic acid or chlorine; for, like that body, it may either be supposed simple, or thought to contain oxygen.

Sir Humphry Davy's first trial was, whether muriate of silver could be formed from it; and he found that the precipitate occasioned by it from nitrate of silver differed from the muriate in being yellow when first formed, and red when fused by a moderate heat. This compound was decomposed by fused hydrate of potash, or solution of potash, and gave an oxide of silver, the oxygen of which is ascribed by the author to the presence of water. This compound of iode and silver was also formed by direct action of the purple vapour on silver leaf, and was found to be red and fusible as in the former experiment.

Potassium heated in the vapour, burns slowly with a pale blue

light, forming a white fusible substance soluble in water, and acrid to the taste, from which iode is again separated by sulphuric acid.

Iode was found to combine with chlorine into a yellow volatile solid, from which iode was again separated by solution of potash, not in excess.

When iode is heated with oxygen gas, or with oxymuriate of potash, it undergoes no change. When heated in the presence of iron, zinc, tin, lead, or mercury, out of the contact of air, it forms compounds that are fusible and volatile, and have a yellow, orange, or red colour, excepting the compound formed with zinc, which is white.

The compound of iode and iron, when exposed to an alkaline solution, yields oxide of iron, but it combines with dry ammoniacal gas without decomposition; whence the author infers that the formation of oxide depends on the presence of water.

When iode is heated in hydrogen, the gas expands considerably, and the compound is found to be highly acid, rapidly absorbed by water, forming a liquid acid without colour, but becoming tawny by dissolving an excess of iode.

Iode combines with phosphorus, producing heat without light; a solid compound is formed that is fusible and volatile; and a strongly acid gas is extricated, that is readily absorbed by water. When potassium or mercury are heated in this gas, they extricate hydrogen equal to half the volume of the gas, and are found combined with iode; so that this gas appears to arise from the presence of hydrogen probably contained in the phosphorus.

When the fusible compound with phosphorus is acted upon by water and heated, much gas arises that is acid and spontaneously inflammable, and the remaining liquid is found to contain hydrophosphorous acid.

When iode is thrown into a moderately strong solution of potash, two compounds are formed, as in the formation of oxymuriate of potash. The first appears in crystals, which form immediately, and fall to the bottom of the solution, and are analogous to hyperoxymuriate of potash, and very similar to it in properties. But the solution contains a different salt, without excess of oxygen, being simple iodate of potash. The crystals are sparingly soluble in water, deflagrate when mixed with charcoal, and yield abundance of oxygen when heated.

By passing the purple vapour over red-hot potash, oxygen is also expelled; and it appears that oxygen quits the triple compound at a red heat.

The affinities of this body for potassium and the metals are inferior to those of chlorine for the same bodies; and accordingly it is extricated from them when the compounds are exposed to oxymuriatic gas.

When the compound of iode with potassium is acted upon by sulphuric acid, a different class of phenomena appear in consequence of the decomposition of the acid, and of the water present. Sulphureous acid is disengaged, mixed with the acid gas formed by the union of

iode with hydrogen; whilst the oxidated base remains in solution in the state of sulphate of potash.

When the same compound is acted upon by nitric acid, nitrous gas and the purple vapour are extricated.

When this compound is acted upon by liquid muriatic acid, it is completely dissolved. The excess of muriatic acid may be driven off by heat, and the acid formed by iode and hydrogen found in the liquor.

When oxy-iodate of potash is dissolved in muriatic acid, muriate of potash crystallizes, and the yellow compound of oxymuriatic gas and iode remains in solution.

When liquid ammonia is poured upon iode, the black fulminating powder observed by MM. Desormes and Clement is produced; and the salt remaining in solution is found by Sir Humphry Davy to consist of ammonia combined with iodic acid, such as was before formed by the union of iode and hydrogen; and he hence infers that the fulminating compound consists of iode and azote, since no azote escapes during its formation.

From several experiments made upon the proportion in which this body unites with potassium, or with potash and with sodium, the author infers that the number representing it will be about 165, that for potassium being 75; and consequently that the acid gas formed by its union with hydrogen must be by far the heaviest known gas.

With respect to the electro-chemical properties of iode, the author observes that it is not decomposed by voltaic electricity from points of charcoal exposed to it in the state of purple vapour; that it is a non-conductor of electricity; that it appears at the positive pole when salts containing it are decomposed in the voltaic circuit, with the exception of its combination with chlorine, from which it is, on the contrary, found to pass to the negative surface.

From all these facts, the author infers that iode is an undecompounded body, resembling metals in specific gravity, lustre, colour, and high elementary weight; in chemical agency and electro-chemical habitudes resembling chlorine, fluorine, and oxygen, having a stronger affinity than oxygen for most metals, but extricated from most of them by chlorine; agreeing with chlorine and fluorine in forming a strong acid with hydrogen; and with oxygen in forming an acid with tin, and substances apparently alkaline, with potassium or sodium, which neutralize dry boracic acid, and form with it a purple glass, from which iode may be separated by sulphuric acid.

In conclusion the author observes, that the acid formed by this body with hydrogen and with tin may be termed hydriodic and stanniodic acids. But for the salts which it forms with various bases, he proposes some termination which shall be merely arbitrary, as Argentama, for the compound it forms with silver; Calcama, for its compound with lime, &c.; so that the fluate, iodate, and muriate of lime are to be distinguished by the appellations of calcala, calcama, calcana.

An Account of a Family having Hands and Feet with supernumerary Fingers and Toes. By Anthony Carlisle, *Esq. F.R.S. In a Letter addressed to the Right Hon.* Sir Joseph Banks, *Bart. K.B. P.R.S.* Read December 23, 1813. [*Phil. Trans.* 1814, *p.* 94.]

These instances of supernatural formation are traced, by the author's inquiries, through four successive generations, from Zerah Colburn, the American calculating boy, to his great grandmother, whose maiden name had been Kendall, but of whose brothers, sisters, or parents, the present generation possess no record.

This woman had five fingers and a thumb on each hand, and six toes on each foot.

She had eleven children, ten of whom are said to have had the same peculiarity complete; but one daughter, the grandmother of the present boy, had one of her hands naturally formed.

Of the next generation there were four persons. Abiah, the boy's father, and two others, had the peculiarity complete; but one of his uncles was like the grandmother, with one hand natural.

The present generation are eight in number, of whom four are naturally formed as their mother is; the rest, including Zerah the calculator, have the peculiarity complete, with the exception of his eldest brother, who has one of his feet naturally formed.

It appears to Mr. Carlisle, that these instances are sufficiently rare to be added to the numerous cases on record of peculiar structures continued by hereditary descent, in the hope that a greater accumulation of facts may enable future physiologists to trace, in some degree, the laws which govern such productions; more especially if attention be paid to the relative influence of the male and female sex in the propagation of peculiarities.

Experiments and Observations on the influence of the Nerves of the eighth Pair on the Secretions of the Stomach. By B. C. Brodie, *Esq. F.R.S. Communicated by the Society for the Promotion of Animal Chemistry.* Read February 10, 1814. [*Phil. Trans.* 1814, *p.* 102.]

Former experiments having shown that when the functions of the brain are destroyed the secretory organs invariably ceased to perform their office, and consequently that the various secretions were probably dependent on nervous influence, it appeared desirable to ascertain this point by dividing the nervous branches by which some one gland is supplied, and observing the effect. But on account of the difficulty of the operation itself, and of the injury done to adjacent parts, it appears extremely difficult to determine the real influence of the nerves in the natural state of all the functions. There are, however, some experiments on the preternatural secretion excited by the action of arsenic, and its interruption by division of the nerves, which the author thinks may deserve to be recorded as tending to elucidate so important a subject.

Mr. Brodie had formerly observed in dogs poisoned by arsenic, a very copious secretion of mucus and watery fluid from the coats of the stomach and intestines, and so rapidly excited, that he conceived this to be a favourable instance for observing the effect of dividing those nerves which supply the stomach.

He consequently divided the nerves of the eighth pair, with the accompanying sympathetic nerves in the neck of a dog, and immediately afterwards inserted ten grains of arsenic into a wound in the thigh. The symptoms which usually appear from the poison of arsenic were soon produced; but though the dog lingered under this treatment three hours and a half, none of that watery mucus observable in other instances of death by arsenic was found in the stomach and intestines, though both stomach and intestines were found much inflamed.

In a second experiment, during nine hours that the dog lingered under the effects of the arsenic applied also to a wound, no such secretion had taken place.

In the third instance, the dog was made to swallow a solution of arsenic, with the same result, after he had lingered three hours.

Since in the preceding trials, respiration was disturbed in consequence of the injury done to the nerves supplying the thorax, a fourth experiment was made by dividing the lower branches of the eighth pair after their passage through the thorax, where they appear in the œsophagus, just above the cardiac orifice of the stomach. In this mode of operating the respiration was not affected; but still the symptoms and visible effects of the arsenic were the same as before, without any fluid evacuations from either the stomach or intestines.

From these experiments, the author thinks it hardly possible to avoid the conclusion, that the suppression of these secretions was owing to the division of the nerves; and that the secretions from the stomach, in general, must be much under the controul of the nervous system. But it appears premature to deduce any conclusion respecting their influence over other secretions.

On a fossil human Skeleton from Guadaloupe. By Charles König, *Esq. F.R.S. In a Letter addressed to the Right Hon. Sir* Joseph Banks, *Bart. K.B. P.R.S.* Read February 10, 1814. [*Phil. Trans.* 1814, *p.* 107.]

The skeleton described in this letter was contained in a mass of stone nearly two tons in weight, brought home by Sir Alexander Cochrane, and presented by the Admiralty to the British Museum. The existence of such skeletons had been mentioned by General Ernouf, in a letter to Faujas St. Fond, published in the fifth volume of the *Annales du Museum*; and also by Lavaisse, in his *Voyage à la Trinidad.* The block brought home by Sir Alexander Cochrane agreed very correctly with the description given by General Ernouf, measuring 8 feet by 2½, having very much the appearance of a huge nodule separated from a surrounding mass, without any marks of a

tool, excepting a few holes that had evidently been made to assist in raising it. The situation of the skeleton in the block was so superficial, that it had probably been discovered by the projection of a part of the left fore-arm. Nevertheless, the operation of laying the whole open to view, with all the care that was requisite for its preservation, was attended with very considerable difficulty, on account of the excessive hardness of the stone adjacent to the bones, and the comparative softness of the bones themselves.

Unfortunately the skull is wanting; and the author, with much reason, regrets the loss of this characteristic part, which by its form might have thrown some light on the period when it was deposited, or at least as to the nation to whom the individual belonged. The vertebræ of the neck are also lost along with the head; the thorax bears marks of violent compression. The seven ribs of the left side are complete, but dislocated. Those of the right side are all broken; and their extremities are found on the left side of the spine. Such parts of the arms and legs as remain, are found in their natural position; but many are entirely wanting, and most are broken, or otherwise imperfect.

The bones are thought to have acquired a degree of hardness since their first exposure, though still far inferior to that of the surrounding stone. A small portion of one of the bones examined by Sir Humphry Davy was found to contain a part of its animal matter, and the whole of its phosphate of lime. The rock in which they are imbedded consists of a calcareous sand, firmly agglutinated. Some of the grains appear to be portions of compact limestone; others are particles of zoophytes; some white, others yellowish; and many which are red in various degrees appear to be fragments of *Millepora miniacea*. Some shells are also found, but in no great number; one of them much resembles *Helix acuta* of Martin; and another is a Turbo, the species of which is not yet determined. The only bony substance observed beside the skeleton itself, has a concentric laminated structure, and appears to be part of a tusk, but from what animal cannot be ascertained. There are also here and there a few specks of a black substance that has the properties of charcoal.

By the workmen employed in exposing the skeleton, the stone is thought to be harder than statuary marble, by the degree of impression made by their saw or chisel. Its formation appears to be similar to that of common sandstone, only that the grains of which it is composed are calcareous, and have in some parts become confluent, particularly in the parts adjacent to the bones, and in these parts Dr. Thomson has found traces of phosphoric acid. From all the circumstances, it is pretty evident that the injury which the bones have sustained has occurred subsequently to their deposition, and before the sand in which they lie had concreted into the present stony substance.

With respect to the period at which this may have happened, the author thinks it impossible to pronounce with decision. It may be of very recent formation, but there is nothing which necessarily im-

plies it to be so. The presence of animal matter is by no means conclusive; since bones from the plaster quarries at Paris still contain it.

Unfortunately, our geological knowledge of Guadaloupe is yet too imperfect to assist in determining this question. The only positive information being, that the bed in which these skeletons are found is nearly an English mile in length, and that it is covered by the sea at high water.

A new Method of deducing a first Approximation to the Orbit of a Comet from three Geocentric Observations. By James Ivory, *A.M. Communicated by* Henry Brougham, *Esq. F.R.S.* Read February 17, 1814. [*Phil. Trans.* 1814, *p.* 121.]

Although it be true that three geocentric observations are really sufficient for determining the parabolic orbit of a comet, as well as the elliptic orbit of a planet; the latter problem is far the easier, because we can select those positions of a planet from which its heliocentric places are found without any intricate calculation: but with regard to comets it is far otherwise. Since their appearance is unexpected, we are under the necessity of drawing our inferences from those positions in which they may happen to present themselves; and it is generally extremely difficult to deduce, with accuracy, their heliocentric positions from observations necessarily confined to a small part of their orbit.

In order to obtain an approximate solution, Sir Isaac Newton considered a small portion of the orbit as a straight line, the projection of which on the plane of the ecliptic will be also straight, and the parts of each will bear the same proportion to each other as the intervals of observation. But three observations alone leave the problem indeterminate; and though when four observations are employed the problem is generally determinate and easily solved, it is also often indeterminate even when four are employed.

In general it may be said that no solution is free from this imperfection, in which the velocity in the orbit does not enter as a principal condition, as in the methods of Boscovich, Laplace, and Legendre. But in that of Laplace, the first and second differential coefficients of longitude and latitude can be obtained but imperfectly, and only by interpolation; and in that of Legendre his formulæ are complicated, and the number of equations that require to be solved render it ill adapted for general use.

The object of the present paper is to give a new solution of the problem, which, in the author's estimation, is at least as accurate as any former method; and in practice, he thinks, as commodious as the nature of such a calculation can well admit.

After detailing the particulars of this method, which from its nature cannot admit of abridgement, the author gives various instances of its successful application in discovering the orbits of the comets of 1769, 1781, and two comets of 1805, from observations selected by Legendre for the same purpose; and he shows, by comparison of his

results with those elements which M. Mechain obtained by Laplace's method, with those obtained by Legendre himself, and with those ultimately deduced as corrected elements from the latest observations, how near an approximation is obtained by the method here given; so that the apparent errors seem rather to be those of observation, which, in fact, are not susceptible of great accuracy even with the best instruments, and with the greatest care, on account of the haze or coma with which these bodies are generally surrounded.

On the Affections of Light transmitted through crystallized Bodies. By David Brewster, *LL.D. F.R.S. Edin. and F.S.A. Edin. In a Letter to Sir* Humphry Davy, *LL.D. F.R.S.* Read December 23, 1813. [*Phil. Trans.* 1814, *p.* 187.]

The present experiments, to which the author has been led by discovering the singular property of agate described in his former communication to the Society, have been attended with results which he considers so extraordinary, that they appear to lead to the very mysteries of physical optics, and exhibit, he says, a series of appearances, which far surpass in splendour and variety all other phenomena of light.

This paper treats, first, of the polarizing power of the agate; secondly, on that structure of the agate on which its properties depend; thirdly, on peculiar colours exhibited by it; fourthly, on the depolarization of light; and fifthly, on certain elliptic coloured rings produced by obliquely depolarizing crystals.

With respect to the polarizing power of the agate, Dr. Brewster has before shown that a ray of light transmitted through a slice of laminated agate, cut at right angles to its laminæ, may be transmitted through a prism of Iceland spar without being subdivided, being refracted ordinarily in one direction, and extraordinarily when the principal section of the spar is transverse to the laminæ of the agate. The author observed at that time a nebulous light that accompanied the bright image of a luminous object seen through the agate, consisting of rays that were not *similarly* affected. He now adds, that this nebulous light is *oppositely* affected, being refracted like the extraordinary rays transmitted through Iceland spar, and accordingly disappearing when the bright image is most discernible, and *vice versâ*. But though the polarization of these rays be different, the refraction of both is the same.

In order to convey, as accurately as may be, an idea of the structure of the agate having these properties, the author assists his description by delineations of the appearances which the substance itself presents in consequence of the variations in fineness of the laminæ, their curvatures, or opacity. Some of the laminæ are white, others transparent; some straight, others variously curved; and where finest and most transparent, exhibiting an appearance of small waves on a surface of water rippled by a gentle breeze, and depending on small variations of the inclination of the laminæ.

In this communication the author again notices certain coloured images, seen on each side of the flame of a candle, or other luminous object seen through the agate, one at $10\frac{1}{2}°$, and a second at $21°$, but which he is not yet able to explain, and supposes to be a new case of production of colours.

Dr. Brewster next gives the result of experiments on the transmission of light previously polarized, through various substances, and notices those positions of crystallized bodies in which the polarization continues unchanged, and those intermediate positions at which complete depolarization takes place; and adds, that such effects are also occasioned by plates of horn, gum-arabic, glue, tortoise-shell, and even plate glass.

In addition to these properties, which mica, topaz, and rock crystal possess in common with other crystallized bodies, Dr. Brewster observes, that they have the power of depolarizing in certain oblique positions, which he considers peculiar to them. And at the same time these bodies have certain oblique positions in which they do not depolarize, and which he terms neutral.

In the preceding experiments depolarization has been effected by the interposition of a second body, through which the rays are transmitted after having been previously modified by some polarizing substance; but the author observes that these effects may both be produced by the same crystals, if the direction of the light be such, that after reflection from the posterior surface it will coincide with the oblique depolarizing axis.

It was in attending to the affections of light thus polarized and depolarized by a plate of topaz $\frac{1}{100}$th of an inch in thickness, that the author observed certain elliptical coloured rings, which he considers entirely new; and as he thinks them important, he takes much pains to describe their various dimensions and successions of colours, and represents them in coloured drawings.

When a doubly-refracting substance is employed to view these rings, the two images seen of them are differently coloured, the colours of one set being complementary to those of the other.

When a plate of agate, or a plane reflector at a specific angle of inclination are employed, then only one or the other set is seen, according to the relative position of the planes of incidence; and it is in the instance of using the plane reflector, that these rings appear with such peculiar brilliancy on account of the absence of all foreign light, which can, in this mode of making the experiment, be completely avoided.

In addition to the above experiments, of which the author gives a detailed account, he remarks, that light reflected at a particular angle from the surface of blue steel is polarized, and thence infers that the oxide is a thin transparent film; that light is partially polarized by reflection from all metallic surfaces.

That light from white clouds or blue sky is partially polarized, but that no part of the moon's light has suffered any degree of polarisation.

On the Polarization of Light by oblique transmission through all Bodies, whether crystallized or uncrystallized. By David Brewster, *LL.D. F.R.S. Edin. and F.S.A. Edin. In a Letter addressed to* Taylor Combe, *Esq. Sec. R.S.* Read January 27, 1814. [*Phil. Trans.* 1814, *p.* 219.]

In examining what changes were produced upon light transmitted through mica in the direction of that line which Dr. Brewster calls its oblique depolarizing axis, he observed some appearances indicating a partial polarization: but upon turning the mica round, so as to preserve the same obliquity of incidence, this effect was found not to depend on the position of the axis, but to be greater or less, in proportion to the obliquity of incidence alone, and to be produced even by a plate of glass substituted for the mica, though not in so great a degree. By transmitting the same pencil of light successively through fifteen plates of glass, at an angle of about 70°, the whole of that which is transmitted is polarized; so that its transmission through agate, its reflection from polished surfaces at a specific angle, or the kind of refraction it undergoes in its transmission through Iceland spar, depend upon the relative position of the planes of refraction. If a second series of similar plates be presented to light thus polarized, it will also be totally transmitted if the plates be parallel to the former, but totally reflected if, with the same inclination, the planes of refraction be at right angles to each other.

By experiments made on the number of plates requisite for causing complete polarization at different angles of incidence, the number varied as the co-tangent of incidence.

The author next endeavoured to ascertain the difference that would be occasioned by using plates of greater refractive density; and he found that a less angle of incidence was then sufficient for effecting complete polarization by the same number of plates: but the subjects of his experiments were not sufficiently different in refractive power for him to determine with precision their proportional effects.

Dr. Brewster observes, that the polarization effected by such a series of plates may be employed with advantage in examining those coloured rings produced by topaz, described in his former communication.

In consequence of the reflections that take place at each of the surfaces, the principal image seen through a number of such plates is always surrounded with a great number of faint images; and when the inclination is very considerable, a nebulous image appears that is oppositely polarized, and has the same relation to the bright image as the author had before observed in agate.

When the coloured rings produced by topaz are viewed through a number of plates so inclined, the two halves of the rings appear completely different; the colours of one set being complementary to those of the other.

When the angle of incidence is 54° 35′ (the angle at which Malus observed *reflected* light to be completely polarized), then the number

of plates requisite for complete polarization of the *transmitted* beam is 30: and since, under these circumstances, the whole of the light that is not reflected at the first surface is transmitted through the whole series, the author observes, that transmission is not in this case a maximum at a perpendicular incidence, and that the law employed by Bouguer fails by reason of these newly-discovered properties of light, of which that distinguished philosopher was not aware.

The celebrated discovery of Malus, of the polarization of light by oblique reflection, and its connexion with the properties of doubly-refracting crystals, is perhaps the most important discovery that has been made in optics since that of the principle of the achromatic telescope; but the author observes, that it does not furnish us with any information of the manner in which these crystals effect polarization, and that the present discovery of polarization by oblique refraction supplies the connecting link between these two classes of facts, and holds out a prospect of a direct explanation of the leading phenomena of double refraction.

Should the present paper meet with the approbation of the Society, Dr. Brewster promises a further communication of experiments on the polarization of light by reflection, in which he designs to show that the law observed by Malus is not general, and that the principle has been completely overlooked by him; as it depends on the proportion which the quantity of light reflected bears to that which is transmitted when incident at the polarizing angle. When light is incident upon water at the polarizing angle, he remarks that only $\frac{41}{1000}$ is reflected; that even from glass only $\frac{78}{1000}$ is reflected; but when realgar, diamond, or chromate of lead are employed, then at the polarizing angle these bodies reflect as much as one half of the light, and consequently have not power to polarize all that they reflect.

Further Experiments on the Light of the Cassegrainian Telescope compared with that of the Gregorian. By Captain Henry Kater, *Brigade-Major. In a Letter addressed to the Right Hon. Sir* Joseph Banks, *Bart. K.B. P.R.S.* Read November 18, 1813. [*Phil. Trans*. 1814, *p*. 231.]

The experiments detailed in the present letter were conducted exactly in the same manner as those detailed by Capt. Kater in his former communication, for the purpose of comparing a new Cassegrainian telescope, made by Mr. Crickmore of Ipswich, with the Gregorian used in the former experiments. The diameter of the large speculum in this instrument is 4·9 inches, but was reduced by a ring of pasteboard to 3·6, in order to render the illumination equal to that of the Gregorian, in which the large speculum measured 3·95 inches.

The areas exposed to the light being estimated at 7·152 and 10·593, and the magnifying powers at the same time being 157 and

125 respectively, the illuminating powers of the two telescopes are inferred to be as 678 to 290.

The mean of this and the two former experiments, without making allowance for the imperfect polish of the Cassegrainian in one of them, gives the comparative superiority in the illuminating power of the Cassegrainian more than 2 to 1; or if the experiment which the author considers less perfect be rejected, it would appear to be 2¼ to 1 in favour of the Cassegrainian construction.

Astronomical Observations relating to the sidereal part of the Heavens, and its Connexion with the nebulous part; arranged for the purpose of a critical Examination. By William Herschel, *LL.D. F.R.S.* Read February 24, 1814. [*Phil. Trans.* 1814, *p.* 248.]

In a former communication to the Society, the author endeavoured to show the probability of a progressive condensation of nebulous matter, so as to put on ultimately the appearance of stars: his present object is to show, by reference to select cases from many thousand former observations on record, that a similar operation of gradual condensation is also taking place among condensed clusters of visible stars, and consequently to render it probable that an intimate connexion subsists between these extremes, and that the same process of condensation continues from one end of the series to the other; so that the most perfect stars may possibly have originated from an accumulation of mere nebulous matter.

His first observations, indeed, relate to a more direct communication between present stars and contiguous nebulosity in different relative positions. In some instances a single star appears to be attracting to itself a branch of nebulosity, seen extending from one of its sides; in others, two adjacent stars appear to have equal power over a linear portion of nebula that extends from one to the other.

The portions of nebula, however, that are adjacent to different single stars, vary considerably in their appearance. Of those nebulous branches that extend only on one side, some have parallel sides, some are fan-shaped, others are in a considerable degree irregular. Some stars have extended nebulosity on opposite sides, in a line of which they occupy the centre. Round others it appears diffused equally, as in a globular form, on all sides; and in some instances, such a globular nebula apparently includes a cluster of several stars together. All these appearances afford a presumption, that stars and nebulæ are drawn together by mutual attraction, and that the accession of such a quantity of matter as must be contained in an extensive nebula will ultimately cause what may be called the *growth* of stars. What in its first state appeared as a globular nebula alone, would, by condensation, present the appearance of a nucleus in its centre. The globular nebula with nucleus would, by increasing condensation, become a nebulous star; its next state would be that of a distinct star, with surrounding nebulosity; and the last result would be the perfect simple star.

The author observes, however, that in the instances which present themselves of such connexion between stars and nebulæ, which are the two extremes of the series, the nebulosity may not always be a remnant of the unsubsided nebulous matter from which they were originally formed, but detached portions of nebulous matter may, like stars, have a considerable proper motion, and may be intercepted in their course by clusters of stars, or by the more powerful attraction of a single star of great magnitude, by which they will in still less time be absorbed. In Dr. Herschel's endeavours to arrange the vast accumulation of observations already recorded on this subject, there are many phenomena too ambiguous to admit of classification; but this, he observes, will necessarily occur at every period in the progressive improvement of telescopes; since a greater power of penetrating into space, which would be sufficient to render all present objects distinct, would bring into view a still greater number of appearances, requiring a still further extension of our powers to comprehend.

After arranging the various instances of gradation in which nebulosity appears successively more and more condensed, whether with or without intervening stars, the author examines aggregations of stars alone, referring to many former observations of patches of stars, which, at the time of recording them, he was induced to call *forming clusters*, in consequence of some appearances of a tendency to approach, which he inferred from the greater density of such clusters toward their centre. This apparent propensity to cluster seemed chiefly visible in parts of the heavens extremely rich in stars; and Dr. Herschel refers to about 150 instances of such an appearance in the Milky Way, but generally of an irregular form, and very imperfectly collected. Of other clusters, in which more of regularity is observable, a more particular description is given.

The various degrees of compression of different clusters are also noticed, with references to numerous instances by classes in which they are now arranged. Some of these are visible with ordinary telescopes; others are selected as fine objects for good telescopes; and others again, on account of their higher compression, cannot be resolved without the aid of very superior telescopes.

The form, also, of those most compressed is observed to partake more or less of a spherical form. Thirty-nine instances are quoted in which the form is oval in various degrees. But objects of this description can hardly be seen to advantage without a twenty-feet telescope. Others again, and very numerous, are referred to, discovered as globular nebulæ with common telescopes, but resolved into stars by telescopes of high magnifying as well as space-penetrating power; and as these are accordingly but little known, Dr. Herschel selects, from numerous observations that he has made during four-and-thirty years, various nebulæ, classed according to the telescopes with which he had observed them, as a guide to those who may wish to view them, that they may be able to judge which objects may possibly be within the power of the telescopes they happen to possess.

Since the luminous appearance of the Milky Way is caused by stars that are invisible to the naked eye, this part of the heavens presents a vast field for observation on the existence of a clustering power. To the naked eye it is visibly divided into large patches; and a telescope shows it to be still further subdivided into unequal groups, which, though now not completely detached, it is presumed will hereafter become insulated; so that the Milky Way will finally be broken up, and cease to be a stratum of clustered stars.

The same mode of reasoning that leads the imagination to conceive the progressive changes of its future existence, involves also the supposition of its origin at some period certainly very remote, but which it may possibly be in the power of *future* astronomers to estimate, by means of accurate observations on the rate of those changes that may be discovered to have taken place in the course of ages yet to come.

With respect, however, to the extent in space of that portion of infinity through which any objects are discernible, and the arrangement and relative distances of all celestial bodies yet observed, the author is of opinion, that some *present* judgement may be formed; and he is now engaged in a series of observations, with a view to investigate the visible extent of the universe.

On a new principle of constructing His Majesty's Ships of War. By Robert Seppings, *Esq. one of the Surveyors of His Majesty's Navy. Communicated by the Right Hon. Sir* Joseph Banks, *Bart. K.B. P.R.S.* Read March 10, 1814. [*Phil. Trans.* 1814, *p.* 285.]

After remarking upon the length of time that has elapsed since any considerable improvement has taken place in the art of ship-building, and the causes that appear to have prevented amendments being introduced, the author gives a general outline of the structure of ships, as hitherto built, which he represents as consisting generally of pieces of timber or plank, all placed nearly at right angles to each other. For, first, the ribs rise at right angles to the keel. The ribs are crossed, on their inner as well as outer side, with planks at right angles to them, and parallel to the keel. And within the inner linings are also a secondary series of ribs, called riders, at some distance from each other, parallel to the former set, and at right angles to the keel. Across this fabric are placed beams, connecting the opposite sides of the vessel; and these also are at right angles to all the parts before mentioned. From beam to beam, at right angles, are the carlings, which support joists parallel to the beams, on which are laid the planks of the deck, in a right line from head to stern, and accordingly preserving uniform adherence to the parallel and rectangular structure, which in every other instance of carpentry is known to every common mechanic to be the weakest form in which any number of lines can be framed together, as it affords no check to that bending of the materials to which they are liable in the direction of their greatest length. To this cause is to be ascribed the well-known

propensity of all large vessels to bend from head to stern when first launched, by reason of the great weight of the two extremities, and the little support at these parts in comparison with the centre, which, from its greater breadth, sustains an over-large proportion of the whole pressure.

This defect of the common structure is shown by reference to a common field gate, which, without the diagonal piece, or brace, would soon fail at every joint, but, when braced, partakes of the advantage of the triangular structure, the principle of which, when correctly applied, occasions the whole stress to be diverted from the transverse direction of the timbers employed, and thrown into that of their length, in which their strength is greatest.

Accordingly, in the new system of ship-building here described, the object of the author has been to introduce diagonal timbers and planks in as many parts of the fabric as could well be accomplished.

In the first place, a diagonal timber is introduced between the upright timbers in each interval between the ports; secondly, instead of the lining, which it has hitherto been the custom to place within the frame, a diagonal timber-work is introduced, intersecting the timbers of the frame at angles of 45°, and about six or seven feet asunder, with their upper ends abutting against what are called the shelf-pieces of the gun-deck beams, and having their lower ends supported by the timber strakes. Between these are also placed other timbers, equally inclined, but in an opposite direction, so as to present a rhombic net-work, which is dowelled to the original frame, and is itself further strengthened by short pieces placed diagonally from corner to corner of each rhombic compartment.

Beside the addition which is thus made to the strength of the sides of the vessel, these sides are also more firmly united together by an improved construction of the knees, by which the transverse beams are united to them. A degree of unity and firmness is also given to the decks by an oblique position of the planks, which, upon the same principle as before, brace the beams and joists, and resist that yielding of the joints which would otherwise take place in their rectangular construction.

The object of the author has been to give as much inflexibility as possible to every part of the hull; for in this part he conceives that unbending stiffness is better calculated than any yielding elasticity to resist those forces to which this portion of the vessel is liable; for though a hope has been entertained, that the known weakness of the common construction might lessen the violence of sudden shocks, by allowing each part to yield in some measure to the force impressed, this conception, he thinks, is not founded on any distinct knowledge of principles, and could not be entertained by those who consider that yielding does not imply elasticity, and who observe, that those parts which yield instead of recoiling with equal force, only become progressively weaker, in consequence of the violence they sustain.

In addition to the foregoing means of bracing by oblique position of timbers and decks, Mr. Seppings has introduced another practice,

which is altogether new, and contributes to the strength on a totally different principle. When a frame-work has the form of a parallelogram, its power to preserve that form depends solely on the strength of the joints; but when the space surrounded by this outline is filled with the same kind of materials, then every part will sustain a proportionate share of any force applied. Accordingly, in the new system, the openings between the ribs are filled in with slips of timber nearly to the height of the orlop, or lowest tier of beams; and when these pieces have been fitted, and closely wedged together, they are caulked and pitched over, so as to make the whole frame, from head to stern, to within a few feet of the greatest draught of water, one compact water-tight mass of timber. Hence, even if any of the outer planking of the bottom were to be knocked off, the ship might not only for a time be kept afloat, but permanently be secured from sinking; whereas, upon the old system, the starting of a plank has been often fatal to the ship and crew.

In addition to these principal improvements of Mr. Seppings, others of less importance are also described, and some observations are added respecting the economy of the new construction, not only with regard to the quantity of timber necessary, but also the quality, and the facility of replacing any parts that may afterwards be found decayed.

Remarks on the employment of Oblique Riders, and on other alterations in the construction of Ships. Being the substance of a Report presented to the Board of Admiralty; with additional demonstrations and illustrations. By Thomas Young, *M.D. For. Sec. R.S.* Read March 24, 1814. [*Phil. Trans.* 1814, *p.* 303.]

Dr. Young observes, that the question respecting the best disposition of the timbers of a ship is by no means so easily discussed as may be supposed by those who have considered the subject but superficially; and deprecates, on the one hand, the forming a hasty determination from a few plausible experiments, as only tending to expose those who are influenced by it to very dangerous errors; and, on the other, the total rejection of the conclusions formed from such experiments without a minute examination of the objections brought against them. He enters into a detailed enumeration of all the force that can act on the fabric of a ship, and into an exact calculation of the probable magnitude of each in such circumstances as are likely to occur; and afterwards considers how far the resistances to be opposed to those forces are sufficient to withstand their action. The strains which occasion the effect of arching are, he observes, of two kinds; the one derived from the distribution of the weight of the ship, with its contents being not duly proportioned to the pressure of the water; the other, which has not hitherto been noticed, from the simple and unavoidable application of the longitudinal pressure of the water to the lower parts of the ship only, amounting to more than one third as much as the former, in the case of a seventy-four gun ship of the usual dimensions, being equivalent to the effect of a weight

of about 1000 tons, acting on a lever one foot in length, while the strain arising from the unequal distribution of the weight, and the displacement, amounts, where it is greatest, to 2600, although it is somewhat less than this exactly in the middle of the vessel. The next force investigated by the author is that of the waves, which he considers as including the consequences of the effect of the wind; and this he finds capable of becoming much greater than the former, amounting, in particular cases of the effect of a series of waves, to a strain of about 10,000 tons, and their difference more than 6000 when the waves are in a contrary direction. Hence it is inferred, that although these occasional strains exceed in magnitude the permanent causes of arching, they do not by any means make it superfluous to give the greatest strength to the fabric in the direction which is best calculated for the prevention of that effect. It is also remarked, that when fastenings have once given way to an occasional force of this kind, the ship must naturally assume the form which is determined by the operation of more permanent causes; and this circumstance may lead the inattentive observer to false conclusions respecting the manner in which the injury has been sustained. The tendency to breaking transversely arises from causes precisely similar to those which have been mentioned as operating longitudinally; but their precise magnitude does not appear to be easily calculable. The force tending to produce a lateral curvature has commonly been in some measure neglected, for want of a permanent strain in a similar direction, capable of exhibiting its effects; but Dr. Young estimates its magnitude, in certain cases of waves striking a ship obliquely, to be nearly or fully equal to that of the vertical strain, as already computed. The manner in which a ship gives way when she strikes the ground is next described; and the effects of partial moisture in promoting decay are mentioned as the last of the evils which it is the object of the builder to obviate, as far as it is in his power.

Dr. Young proceeds to consider the arrangements that are best adapted to obviate the various strains which are likely to occur in the fabric of a ship, and observes, that the principal, if not the only, advantage of oblique timbers is in the additional stiffness which they afford; since the ultimate strength, or the resistance at the point of breaking, is little, if at all, affected by them in the cases which have been proposed for experimental examples, though, in some other cases, the strength as well as the stiffness may be surprisingly increased by the obliquity of the substances employed. In a ship, the utility of oblique timbers must depend in great measure on the changes which are observable in cases of arching, whether they consist most in an alteration of the angular situation of the parts, or in the want of continuity from a failure of the fastenings. From actual observations made at Chatham, he concludes that half of the effect produced depends on one of these causes, and half on the other; and infers, that so far as a change of the angular position of the timbers is found to take place, the addition of oblique braces must be of the

greatest utility; an opinion in favour of which he adduces the authorities of Bouguer, Gobert, and Don George Juan. He then proceeds to calculate how far Mr. Seppings's braces are strong enough to sustain alone the force to which it has been proved that their situation is likely to expose them; and finds that they will support, without being crippled, such a change as may be expected when a seventy-four arches about two feet, but not more; and that they will afford a resistance fully sufficient to withstand a strain much greater than that which has been attributed to the pressure of the waves, and to the usual causes of arching. Dr. Young does not apprehend any evil from the omission of the internal planking between the parts, nor from the removal of the partial remedy which the immersion of the ends, produced by arching, affords to the unequal distribution of the weight and pressure. The filling-in between the timbers in the hold he considers as wholly unexceptionable; and remarks, that wedges may easily be driven in such a manner, while the ship is on the stocks, as to have a tendency to render the keel convex rather than concave below, and to prevent the common effect of arching when the ship is launched, without any other superiority of strength or workmanship; and that, without some such accidental cause, no ship when launched could be wholly free from a perceptible degree of arching. He doubts the superiority of Mr. Seppings's iron fastenings of the beams when acting as ties; and observes, that the obliquity of the planks of the decks diminishes in some degree the strength of the tie with respect to arching; but remarks, that it may perform a very important service in rendering the ship more capable of resisting the lateral strains, which, although sometimes very violent, have been little considered by theoretical reasoners: and he suggests that it may be possible to fix the carlings between the beams in such a manner as to contribute more materially to the strength in this respect. In case of the ship's grounding on a hard bottom, Dr. Young is disposed to think Mr. Seppings's construction somewhat weaker than the common one, on account of the omission of the ceiling; although an experiment made on the Tremendous proved that a force more gradually applied could be sustained without injury. And he concludes from the whole examination, that none of the objections which have been hitherto advanced appear to be sufficiently valid to warrant a discontinuance of the cautious and experimental introduction of Mr. Seppings's arrangements, which has been commenced by order of the Board of Admiralty.

Some further Observations on Atmospherical Refraction. By Stephen Groombridge, *Esq. F.R.S.* Read March 31, 1814. [*Phil. Trans.* 1814, *p.* 337.]

In the author's former communication to the Society on the subject of atmospheric refraction, he considered the observations of stars that were more than 80° from the zenith as not to be sufficiently depended upon for the determination of refraction in general; and

accordingly, in his computation of a formula for that purpose, η Ursæ Majoris, at 70° 10′ zenith distance, was the lowest star included in his estimate. By applying that formula to stars below 80° zenith distance, Mr. Groombridge has since found some correction to be necessary; for the refraction at the low altitudes is not really so great as might be presumed from that of stars less distant from the zenith. He has consequently been induced to make a course of observations on other circumpolar stars, beyond the former limits, and as near to his north horizon as the situation of his observatory would permit. Since the formula of Dr. Bradley appears most conveniently applicable to the purposes of the practical astronomer, but not quite correct in the numbers assumed for refraction at 45°, and for the coefficient of x, the author has endeavoured to find out such numbers as would correspond more accurately with observation, and has found that the same formula may be made to serve as low down as to 87° of zenith distance, by increasing the amount of the numbers before mentioned still a little more than he had formerly done.

But for stars of less altitude than this, or within three degrees of the horizon, he found that the same formula could not be made to serve throughout, but that it became necessary to vary the amount of the coefficient y nearly in proportion to the excess above 87° zenith distance.

The table of observations on which these estimates are founded is divided into two parts; the former of which is the result of more than 240 observations made upon sixteen stars between 80° and 87° zenith distance; and the latter is founded upon six other stars between 87° and 88° 42′ zenith distance.

The difference of the obliquities of the ecliptic, as deduced from the summer and winter solstices, or disagreement between the elevation of the equator, thence deduced, and the zenith distance of the pole, as inferred from the zenith distance of circumpolar stars, is ascribed by the author to error in the quantities of refraction, which was assumed too small by Dr. Bradley. And he remarks, that, on the contrary, his observations of the solstices, when reduced according to his own improved formula for refraction, agree in giving results that correspond with his observations of circumpolar stars.

With respect to thermometrical corrections, Mr. Groombridge observes, that his results appear to be most correct when reduced by the state of the thermometer without-doors instead of that which is within; and that the difference is very considerable where the zenith distances are so great as those included in the present remarks.

Propositions containing some Properties of TANGENTS *to* Circles; *and of* TRAPEZIUMS *inscribed in Circles, and non-inscribed. Together with Propositions on the* Elliptic *Representations of* CIRCLES, *upon a plane Surface, by* PERSPECTIVE. *By* Richard Hey, *LL.D.*; *late Fellow of Sidney Sussex and Magdalen Colleges, in the University of Cambridge. Communicated by the Rev.* Edward Balme, *M.A. F.R.S.* Read March 31, 1814. [*Phil. Trans.* 1814, *p.* 348.]

That the perspective representation of an entire circle is an ellipse, is simply a part of the doctrine of conic sections, and is not in need of demonstration. The principal inquiry of the author has been with regard to the positions of the axes of such ellipses.

Selecting the case of any number of circles in the same plane, and having their centres in the same line, he examines what law is to be observed in the directions of the axes of the representing ellipses.

It is shown first, that, excepting one particular case, their axes do not converge to one point, but are parallel to other lines that do converge to one point. He does not, however, pretend to determine accurately and separately each of these other lines, but endeavours to ascertain certain limits within which they must be arranged, and a certain regularity in the variation of their directions, which he considers sufficient for all the practical purposes of the artist.

Preparatory to this inquiry are a series of propositions purely geometrical, relating to the properties of tangents to circles, and of trapeziums inscribed in circles, some of which may not be altogether new, but are inserted as necessary to the demonstration of other properties, which he believes have not before been noticed. These are kept separate from the perspective propositions, because they may be more interesting to mathematical readers than they might be if interwoven with considerations of their application to perspective representation. The greatest part of this paper of course could not admit of being publicly read, being unintelligible without reference to the figures which accompany it.

On new Properties of Light exhibited in the optical Phenomena of Mother-of-Pearl, and other Bodies to which the superficial structure of that Substance can be communicated. By David Brewster, *LL.D. F.R.S. Edin. and F.S.A. Ed. In a Letter addressed to the Right Hon. Sir* Joseph Banks, *Bart. K.B. P.R.S.* Read April 28, 1814. [*Phil. Trans.* 1814, *p.* 397.]

The first section of this paper is devoted to the optical properties peculiar to mother-of-pearl; the second to the communication of these properties to other bodies; the third to the consideration of the cause of these phenomena; and the fourth to the description of the peculiar species of polarization produced by this substance.

Dr. Brewster observes, mother-of-pearl is composed of laminæ, much resembling in their arrangement those of the agate; that when it is imperfectly polished, a coloured image of a candle is seen in it

by reflection in the neighbourhood of the common image, having its blue extremity towards this image, and being always situated, with respect to it, in the direction of an axis of extraordinary reflection, the angular distance varying with the inclination and situation of the rays, and being also different in magnitude in different specimens, but always observing certain laws. There is also a mass of coloured light, crimson at great angles of incidence, and green at smaller, beyond the regular coloured image, its distance varying according to a different law; becoming brighter when the substance is polished, and varying also with its thickness. Similar appearances are observed in a surface obtained by fracture; but a higher polish produces a new coloured image on the opposite side of the common image, and nearly as bright as the former, which is rendered somewhat less brilliant by the operation of polishing. Similar appearances, but somewhat less distinct, are observed when a candle is viewed through a thin piece of mother-of-pearl; and it is remarkable, that the image which is the brighter when seen by reflection, is the less bright when seen by transmission. When the opposite surfaces happen to have different axes of extraordinary reflection, they produce the appearance of four images in the transmitted light.

Dr. Brewster having had occasion to fix a piece of mother-of-pearl, by a hard cement, to a goniometer, was much surprised to find that the cement had acquired properties, with respect to colour, nearly similar to those of the mother-of-pearl; and he afterwards succeeded in producing the same effect with black and red wax, balsam of Tolu, gum arabic, gold leaf placed on wax, tin foil, fusible metal, and realgar; and, by means of pressure, with lead; the appearances exhibited by these substances varying also, like that of the mother-of-pearl, from which they were derived, according to the degree of polish. But the mass of crimson and green light never accompanies these appearances: and, on the other hand, it is produced by mother-of-pearl, even when its reflection is destroyed by the contact of a substance of equal refractive density; so that it appears to depend on the internal constitution of the mother-of-pearl. The colours seen by transmission are more brilliant in gum arabic which has received the impression, than in the original substance, on account of the difference of transparency; and the refractive density of the substance employed for the impression does not appear to have influenced the magnitude of the dispersion, as exhibited by the coloured images. Pearls also were found to communicate their properties to other substances in a similar manner, the principal image being surrounded with a nebulosity, which is observable in an impression on wax.

Hence Dr. Brewster very naturally inferred that the peculiar phenomena of mother-of-pearl are owing to a particular configuration of its surface; and he had the satisfaction to find this inference fully confirmed by microscopical researches. The surfaces are almost always visibly grooved, so as somewhat to resemble the skin of the fingers: the grooves are sometimes perceptible to the naked eye, but sometimes too fine to be discovered even with magnifying powers

of 300 or 400 times, and no polishing can remove or modify them. Sometimes 3000 of them may be counted in an inch, while in another part of the same specimen they become much coarser; and a corresponding appearance may be detected in the substances which have received the impression. The axis of extraordinary reflection is always perpendicular to the direction of these grooves. Dr. Brewster thinks that the general reflection by which the common image is formed, is the effect of the repulsive force of the whole surface, acting at such a distance, that its irregularities do not interfere with the equality of the angles of incidence and reflection; but that which has escaped this reflection is subjected to the influence of the grooves, the form of which, as he shows from the phenomena, must be curvilinear. The spectra do not resemble those which are produced by ordinary inflection, and which are observed in circumstances materially different. Dr. Brewster has in vain attempted to obtain coloured impressions from the Labrador spar, and from several of the metallic oxides: he finds that the crimson and green light of the mother-of-pearl are dependent on its thickness, like the colours which are seen in the common thin plates, but that they appear at much greater thicknesses.

The last peculiarity which the author has found in this interesting substance, is the manner of its polarizing light. In crystallized bodies, two portions of light are differently polarized with respect to the direction of the plane of incidence; but here the transmitted and reflected light are polarized in the same direction. The polarization is the most complete when the angle of incidence is about 60°, and when the thickness is about one fortieth of an inch; the transmitted light is in this case wholly polarized. If the plate is thicker, the transmitted light is wholly polarized at a smaller angle of incidence; and this polarization remains unaltered when the superficial reflection is destroyed by the contact of a substance of equal refractive density.

Dr. Brewster concludes with observing, that the subject is far from being exhausted; and that if the investigation could be carried on with the aid of analogous phenomena, we might confidently look forwards to some great change in the fundamental principles of physical optics.

An improved method of dividing Astronomical Circles and other Instruments. By Capt. Henry Kater. *Communicated by* Thomas Young, *M.D. For. Sec. R.S.* Read May 5, 1814. [*Phil. Trans.* 1814, *p.* 419.]

Although the author is most ready to admit the accuracy of Mr. Troughton's method of dividing, which was described in our Transactions for the year 1809, and by which the mural circle at Greenwich has been divided, it appeared to him that some improvement might be made in regard to simplicity and facility of execution.

The general principle of the method here described by Capt. Kater is, in fact, the same as that of the beam compass; but his apparatus,

instead of having points like the beam compass, has two micrometer microscopes adjustable to different distances, as aliquot parts of an arc or line to be divided. For the purpose of marking on the surface of the instrument any assumed interval between the microscopes, certain thin pieces of small brass, carrying dots, are employed, with a clamp for fixing them to any part of the limb, and with proper screws for adjusting them to any precise position.

In addition to these, which are adapted for finding aliquot parts of the circle under division, the apparatus is provided with a cutting point of the usual construction, and a third microscope fixed always in the same position, for the purpose of observing a certain line first laid down as a permanent point of reference, and for watching the performance of the cutting-point, while it marks the successive intervals, determined by means of the two adjustable microscopes.

As a specimen of the method in which this apparatus is to be used, the author describes the series of divisions and subdivisions which he thinks most convenient in a circle of two feet diameter.

He first divides into five parts of 72° each.

At the same time that the fixed microscope is kept steadily over the original line of reference, two adjustable dots are placed, by estimation, at the distance of 72° from each other, and in such a position with reference to the cutting-frame as not to interfere with its motion. The two adjustable microscopes having next been placed so that their wires exactly correspond with these dots, the circle is made to revolve so that the second dot shall come under the first microscope when a third dot is to be placed under the second microscope; and in the same manner a fourth and a fifth dot in succession, till the fifth interval can be compared with the distance between the microscopes, when the first dot should be found to correspond exactly with the second microscope: if it be otherwise, the difference must be measured by the micrometer; and the distance between the two micrometers, originally assumed at 72°, must be corrected by one fifth part of the error so found, and must subsequently be ascertained to be correct, by a careful repetition of the same operation of the adjustable dots. When the fifth interval has thus been made accurately to correspond with that between the adjustable microscopes, the original point of reference is then to be returned to its position under the fixed microscope, and the divisions are to be marked in succession as the series of dots are made to appear under the wires of the adjustable microscopes, which are now known to be accurately 72° from each other.

The next step taken is to divide each of these intervals into three parts of 24 each, and again trisect into parts of 8°. These, by repeated bisection, are reduced to half degrees; and these again, by trisection, are divided into spaces of 10′ each.

In the performance of the latter steps of this subdivision, the author suggests the periods when it may be found convenient to bisect some larger odd number of divisions, instead of taking a single one for bisection, on account of the interference of the microscopes with each

other; and he takes care to mention such other precautions as may not immediately occur to artists in the employment of a new apparatus, and to delineate accurately all those parts which might not be thoroughly understood by a mere verbal description.

Results of some recent Experiments on the Properties impressed upon Light by the Action of Glass raised to different Temperatures, and cooled under different Circumstances. By David Brewster, *LL.D. F.R.S. Edin. and F.A.S.Ed. in a Letter to the Right Hon. Sir* Joseph Banks, *Bart. K.B. P.R.S.* Read May 19, 1814. [*Phil. Trans.* 1814, *p.* 436.]

The author, being engaged in making a variety of experiments on resinous and other bodies that could be fused between plates of glass, remarked a partial depolarization while the subject of examination was hot, but which diminished on cooling, and consequently could not be ascribed to incipient crystallization. He therefore tried a plate of glass alone; and having previously raised its temperature almost to a red heat, he found that a ray of polarized light became completely depolarized by its passage through it: and he further thence infers, that glass brought to a certain temperature forms two images, and polarizes them like all doubly refracting crystals, only that the two images are, in fact, coincident, instead of being separated.

Since in the formation of the glass-tears, called Prince Rupert's drops, which are made by dropping melted glass into cold water, it is probable that in consequence of the sudden consolidation at the surface, the interior part is prevented from contracting, and consequently retains, in some measure, that relative distance of its particles which obtained in the fluid state, the author conceived these drops to be a fit subject for an interesting experiment; and having procured several such drops, made of white flint-glass, he cut and polished one of them by two planes at right angles to the axis, and a second by two planes parallel to its axis and to each other. When polarized light was transmitted through a drop in either of these directions, it was found to be depolarized; but there was not any position in which the transmitted ray would retain its polarization, as is found in corresponding experiments with crystallized substances.

Consideration of various Points of Analysis. By John F. W. Herschel, *Esq. F.R.S.* Read May 19, 1814. [*Phil. Trans.* 1814, *p.* 440.]

This paper is divided into four sections, the first of which treats of the calculus of generative functions, and relates solely to characteristic notation, and to the method of separating the symbols of operation from those of quantity. The second relates to logarithmic transcendants, with a variety of remarkable results deduced from them. The third relates to functional equations. The fourth to differential equations of the first degree. But the whole of this paper was of a nature not adapted for public reading.

Observations on the Functions of the Brain. *By Sir* Everard Home, *Bart. F.R.S.* Read May 26, 1814. [*Phil. Trans.* 1814, *p.* 469.]

The observations comprised in this paper, are selected from those cases of injury to the brain which have occurred to the author in the course of his professional pursuits. The facts thus accidentally forced upon his notice, may be regarded as so many experiments made on different portions of the living brain; and the remarks upon them relate to those effects which tend to elucidate their several functions.

The collection of observations here given, are classed under different heads; and with respect to the first set, which relate to the pressure of water on the brain, the subject is again subdivided according to the parts in which the water may be collected, whether in any of the ventricles, or between the membranes. In the next place, the consequences of concussion of the brain generally, are also considered.

The effects of extravasation of blood, in various situations, are separately described. The consequences that ensue from formation of matter, and immediate relief of the symptoms by its removal, are noticed.

The symptoms that occur from depression, or from thickening of different parts of the skull, are next distinguished, as well as those which arise from pressure of soft tumours in different situations.

In addition to the preceding, which are all instances of pressure variously modified, the author adds his observations relating to wounds, inflammation, and suppuration of the cerebrum in different parts; and his remarks upon injuries done to the medulla spinalis, which form the concluding section of his classification.

Further Experiments and Observations on Iodine. *By Sir* Humphry Davy, *LL.D. F.R.S. V.P.R.I.* Read June 16, 1814. [*Phil. Trans.* 1814, *p.* 487.]

The present set of experiments are, in part, a continuation of the author's experiments on compounds of iodine and fixed alkalies, which he treats of under the head of triple compounds, because they contain iodine, oxygen, and potassium, or sodium. But he also treats of various compound salts, which this substance forms in conjunction with other acids, and of the effects produced upon iodine by the action of some compound gases.

When the triple compound of iodine, oxygen, and potassium, is dissolved in nitric acid, the acid may be distilled without any decomposition of the salt; but when it is dissolved in sulphuric acid or phosphoric acid, the heat which these acids will bear is sufficient to decompose the salt, which then yields oxygen and iodine, and leaves sulphate or phosphate of potash.

When a solution of this salt, in strong muriatic acid, is heated, there is a smell of chlorine, the fluid becomes yellow, and yields, by distillation, chloriodic acid.

A solution of the triple compound, in sulphurous acid, when distilled, yields iodine, and leaves sulphate of potash; but the phenomena are variously modified, by a greater or less proportion of either ingredient.

From such experiments as the author has made on the proportional weights of the constituents of this salt, he considers it perfectly analogous to hyperoxymuriate of potash; and its constitution will be represented by one of potassium 75, six of oxygen 90, and one of iodine 165.

The author also made triple compounds with iodine and alkaline earths, which, like oxyiode of potash, contain a redundance of oxygen, separable by heat, and gave hopes that a compound of iodine and oxygen, similar to euchlorine, might be obtained from some of them; but these salts are not decomposable by acids; for even the oxyiode of barytes is not decomposed by sulphuric acid, and hence no compound of iodine and oxygen has yet been obtained in a separate state.

From hydriodic gas, or from the acid formed by union of this gas with water, iodine may be obtained by union with oxygen, by nitric acid, by hyperoxymuriate of potash, or even by absorption of oxygen from the atmosphere.

This acid unites with the alkalies and common earths into compounds very analogous to the corresponding compounds with muriatic acid, but decomposable, in a certain degree, by heat when oxygen is present, which occasions most of these compounds to become alkaline when long heated.

Although chlorine and iodine unite in all proportions, there is one compound nearly colourless that appears to be definite, having strongly acid properties, and the author terms this chloriodic acid. When any of these compounds are mixed with alkaline solutions, the tendency appears to be, in the first instance, to form oxyiodes with the alkali or earth present; but the phenomena necessarily vary according to the proportion of the several ingredients present.

In the next set of experiments, which the author made with gases, the results appear to be regulated by the presence of hydrogen, forming hydriodic acid with the iodine, as in the instances of olefiant gas and sulphuretted hydrogen.

No change was produced in nitrous gas, nor in carbonic oxide, to which iodine was exposed in common day-light, nor even when it was sublimed in it; but it appeared doubtful whether there might not be some tendency to combine when the violet vapour was formed by heat in full sunshine.

In conclusion, the author reports various unsuccessful attempts to obtain iodine from different species of sea-weeds, and from sea-water, on the shores of the Mediterranean; and he recommends silver as a test of its presence, since water containing only $\frac{1}{1000}$th part of its weight of any salt of iodine tarnishes polished silver, even after boiling with muriatic acid, although this property is destroyed in sulphurets by similar treatment.

The sea-weeds tried by the author amount to six species.

Fucus cartilagineus,	*Fucus filamentosus,*
—— *membranaceus,*	*Ulva pavonia,*
—— *rubens,*	—— *linza.*

But he could discover no traces of it in any of these, nor in certain corallines and sponges which he also tried.

Observations respecting the natural production of Saltpetre on the walls of subterraneous and other Buildings. By John Kidd, *M.D. Professor of Chemistry at Oxford. Communicated by* William Hyde Wollaston, *M.D. Sec. R.S.* Read June 16, 1814. [*Phil. Trans.* 1814, *p.* 508.]

The intention of the present paper is to state the result of a series of observations on the connection of production of nitre with the state of the atmosphere; and the account begins with a description of the situation of the laboratory of the Ashmolean Museum, where these observations were principally made; the pavement being nine feet below the level of the street in which the museum stands, and seventeen below the highest part of its ceiling, which is arched, and, as well as the side walls, consists of a calcareous freestone.

The saline efflorescence takes place principally on three sides that are surrounded by high ground, and but little on the fourth side, where the ground without is on a level with the pavement.

It is observed, that even in the midst of those parts that abound most in nitre, there are certain places which always produce much less than others; and even insulated patches, which are always and entirely free from any appearance of efflorescence, showing that these gradations depend on some difference in the texture or composition of the stone. It is also remarked, that such differences are often not by gradual transitions, but occur abruptly at the passage of a line, on one side of which there appears an abundant crop, and on the other never the slightest efflorescence; but this does not depend on the joints of the masonry, but takes place indifferently on the surface of the stones composing the wall, and of the mortar by which they are cemented.

With regard to the influence of different states of the atmosphere on the production of nitre, Dr. Kidd observes, that it is most abundant in clear frosty weather, and that in a moist state of the atmosphere the formation either does not take place, or goes on very slowly. Sometimes also, that which has already formed disappears, as if the moisture occasioned it to be re-absorbed into the substance of the wall: but the author was not able to detect it in a portion of the stone taken from near the surface, and lixiviated for that purpose; and he also observes, that it occasionally disappears in dry frosty weather, when no absorption could be supposed to take place.

Wishing to ascertain whether the presence of atmospherical air was necessary to the production, the author coated a productive part

of the wall with a plate of glass, well cemented round on all sides; and it appeared evident that the whole quantity formed under these circumstances, was nearly equal to that which usually formed on the same surface when exposed to the free action of the atmosphere.

The author concludes this paper with an analysis of the stone of which the laboratory is built, showing that it contains 96 per cent. of carbonate of lime, the rest being sand, oxide of iron, ochry clay, with a trace of animal matter, which is conceived to be from the shells contained in the stone.

He also gives the result of his experiments on the nitre collected in this situation, which shows that the quantity of calcareous salt contained in it does not exceed $\frac{1}{100}$th part, instead of being a principal constituent, as authors have asserted.

On the Nature of the Salts termed triple Prussiates, and on Acids formed by the union of certain Bodies with the Elements of the Prussic Acid. By Robert Porrett, *jun. Esq. Communicated by* William Hyde Wollaston, *M.D. Sec. R.S.* Read June 30, 1814. [*Phil. Trans.* 1814, *p.* 527.]

Although it be very well known that the properties of the triple prussiates depend on the presence of an oxide of iron, the differences between these and the simple prussiates in being neutral, and with difficulty decompounded, are by no means explained; and the object of the author is to reconcile these anomalies with the general properties of other saline bodies.

The facts observed by him have led him to consider the salts, hitherto termed triple prussiates, as binary salts consisting of a single base, combined with a very compound acid, in which iron enters as a constituent along with the elements of prussic acid. The leading experiments on which this opinion is founded are two; first, the decomposition of a triple prussiate of soda by the voltaic battery, which occasions the alkali to go alone to the negative pole, and carries the iron not to the negative as a base, but to the positive pole, as one of the elements of the acid part of the salt. In a second experiment he decomposes a triple prussiate of barytes by sulphuric acid, and obtains a fluid having all the characters of an acid, which forms directly with alkalies, earths, and oxides, the salts termed triple prussiates, and by superior affinity displaces carbonic and acetic acids from their combinations.

By distillation this acid may be decomposed into prussic acid and oxide of iron, which has therefore been thought to be present as a base, by those who have overlooked the circumstance of the compound being acid, and in fact a much stronger acid than the prussic acid itself. Accordingly, when it is not exposed to too great a heat, this acid is transferred entire from one base to another, in many instances, of double decomposition, and produces effects altogether dissimilar to those of mere prussic acid.

The author observes also, that there are other substances beside

oxide of iron that are capable of forming peculiar acids with the elements of prussic acid; and the most remarkable of these is sulphur, which makes an acid of a red colour, having always the same properties, though formed in various different ways. The first method by which Mr. Porrett formed this acid, was by boiling together sulphuret of potash with prussian blue; but he has also made it by the same sulphuret with prussiate of mercury. Also by heating together sulphuret of potash with animal charcoal, and by sulphate of potash with the same coal. Also by boiling a simple alkaline prussiate with sulphur, or by mixing prussiate of ammonia with hydroguretted sulphuret of potash. Since one of the properties of this acid is to form an insoluble compound with copper, the author takes advantage of this valuable property for obtaining the acid in a pure state. After decomposing a salt of copper for this purpose, a quantity of sulphuric acid is poured on the precipitate, and the whole submitted to gentle distillation, by which the acid is obtained nearly pure, or may be easily purified. The author examines the salts formed by union of this acid with the several alkalies, earths, and metallic oxides, showing that it may be transferred from one to another without change of its properties, and supporting his opinion that it should be regarded as an acid of a peculiar and extremely compound nature.

For the acids here described, the author invents names by combining the initials of three of their constituents, carbon, hydrogen, and azote, which give him the term Chyazic; and hence he denominates the former Ferruretted Chyazic acid, and the latter Sulphuretted Chyazic acid. By careful analysis of the former, the author found 17·26 oxide of iron in 47·66 of the dry acid; and in 18·4 of the latter he found 12 of sulphur.

In the course of these experiments Mr. Porrett examines and describes the precautions which are necessary in ascertaining the quantity of iron present in any solution by the quantity of prussian blue that can be formed: and he also shows the use that may be made of the sulphuretted chyazates as precipitants of copper. He observes, that the precipitate formed in this case contains no water, and consists of about 63 protoxide of copper, combined with 37 sulphuretted chyazic acid.

Some Experiments on the Combustion of the Diamond and other carbonaceous Substances. By Sir Humphry Davy, *LL.D. F.R.S. V.P.R.I.* Read June 23, 1814. [*Phil. Trans.* 1814, *p.* 557.]

Notwithstanding the many accurate experiments which have been made and recorded, showing that diamond and carbonaceous substances combine with the same quantity of oxygen, and form the same quantity of carbonic acid, various conjectures have been formed respecting some difference in their chemical composition, which might account for the remarkable difference in various sensible qualities. Messrs. Biot and Arago conjectured, from the great refractive power of the diamond, that hydrogen must be present. Guyton de Morveau

imagined that other carbonaceous substances were oxides of diamond; and Sir Humphry Davy himself supposed, on the contrary, that diamond, as a non-conductor of electricity, probably contained oxygen, and afterwards that it contained some new principle of the same class with oxygen.

Having, however, lately made some direct experiments on the combustion of the diamond in oxygen gas, by means of the great lens belonging to the Academy at Florence, his results have not differed from those made by Mr. Tennant, and subsequently by Messrs. Allen and Pepys, respecting the quantity or quality of the gas produced; and he acknowledges that the general tenour of his experiments is opposed to the conjectures that have been entertained by himself and others respecting the existence of oxygen, either in the diamond itself, or in carbonaceous substances. His experiments likewise, so far from supporting the hypotheses of Messrs. Biot and Arago as to the existence of hydrogen as a constituent part of diamond, showed that a minute quantity of hydrogen was really contained in each of the other carbonaceous substances employed for comparison, not excepting plumbago. The presence of hydrogen in these bodies is most distinctly shown on heating them in chlorine, by white fumes that are immediately perceived in consequence of the production of muriatic acid; but when diamond is heated in the same gas, no such vapour appears. In the course of these experiments the author notices a phenomenon which *he* had not before *seen*, namely, that diamond when once ignited in oxygen, continues to burn till it is consumed.

Some Account of the fossil Remains of an Animal more nearly allied to Fishes than any of the other Classes of Animals. By Sir Everard Home, *Bart. F.R.S.* Read June 23, 1814. [*Phil. Trans.* 1814, *p.* 571.]

The bones here spoken of, are from the cliff between Lyme and Charmouth in Dorsetshire. The cliff, says the author, is composed of limestone, upon which is a stratum of blue clay two or three feet thick, in which these bones were deposited.

A drawing has been made of these bones to accompany the paper, which supersedes the necessity of a very particular description. Their magnitude is such, that the head alone measures four feet. The upper and under jaw are very distinct, set with small conical teeth, as in the crocodile; but the lower jaw is not articulated as in that animal, but connected by an intermediate flat bone, as in fishes. The sclerotic coat of the eye is also, as in fish, bony, but is subdivided, as in the eyes of many birds, into a number of separate plates. The intervertebral cavities of the spine likewise prove, that this skeleton is that of a swimming animal; since the form of each cavity is that of an oblate oval, much wider in its transverse diameter than in the direction of the spine. The mode of articulation of the lower jaw, which admits of its being opened to a great extent, seems to show the animal

to have been voracious, as would appear also from the structure of the teeth; but the points in which it differs from any one animal, and resembles others belonging to classes extremely remote, occasion the author to view it, with the singular productions of New South Wales, as one of the connecting links in the creation, formed for the purpose of preventing any void in the chain of imperceptible gradations, from one extreme of animated beings to the other.

On an easier Mode of procuring Potassium than that which is now adopted. By Smithson Tennant, *Esq. F.R.S.* Read June 23, 1814. [*Phil. Trans.* 1814, *p.* 578.]

The process originally discovered and described by Messrs. Gay-Lussac and Thenard for obtaining potassium by means of iron, requires that the iron should at first be intensely heated, and afterwards that the alkali should be applied to it in the heated state. For this purpose a gun-barrel is required of such a length as to pass through a furnace purposely constructed, having at its extremity a second short portion of barrel neatly fitted to it by grinding, for the purpose of containing the alkali; and from which it may be made to flow by means of a separate fire, to be applied by the attendant operator at such a stage of the process, and at such a rate, as is judged to be most advantageous.

Since in this method, though the alkali is, in fact, soon mixed with the iron, the process nevertheless requires the heat to be continued for nearly an hour, the author conceived that nearly the same effect might be produced merely by mixing the same ingredients previously, and distilling them in the following simple apparatus.

A straight gun-barrel, coated well at its lower part with Stourbridge clay, is filled to about one half its length with a mixture of iron turnings and potash. Into the upper half of this barrel is inserted a smaller and thinner tube of iron, contracted at its lower extremity to a small orifice, sufficient to admit the vapour of potassium to pass, and of such a length that its upper extremity may project a little beyond the end of the gun-barrel; and then both are covered at the same time by a cap, which fits the gun-barrel sufficiently to be closed with cement. In the top of this cap is a cork, with a tube of safety for passage of gas that escapes during the operation.

The advantage of the inner tube, in which the potassium is received, consists not merely in the facility with which the product is withdrawn, but in preventing an admixture of potash, with which it is otherwise liable to be contaminated.

On the influence of the Nerves upon the Action of the Arteries. By Sir Everard Home, *Bart. F.R.S.* Read June 30, 1814. [*Phil. Trans.* 1814, *p.* 583.]

The object of this paper is to show that the nerves which accompany the arteries regulate their actions, and occasion different pro-

portions of blood to be supplied to different parts of the body. The facts which first led the author to entertain this opinion, were the accidental consequence of an extremely painful application of pure kali to a wound, which occasioned a general pulsation of the limb to which it was applied, although the pulsations of distant arteries were at the same time undisturbed. In order to be quite certain that this consequence was really dependent on the irritation of nerves, the author made two experiments on rabbits in the neighbourhood of the carotid artery. Having laid bare the par vagum and intercostal nerves, a probe was passed under the former so as to separate it, so that the irritation might be first given to this nerve alone; but no sensible effect was thus produced upon the artery. But when the same application of pure kali was made to the adjacent intercostal nerve, by which the artery is supplied, the dilatations and contractions of the artery were considerably increased, and the violence of the pulsations continued about three minutes before they began to subside.

The same experiment being repeated on a second rabbit, was attended with the same result; and it was afterwards repeated on a dog without any perceptible difference.

These visible effects of the influence which the nerves possess over the arteries, enable the author to comprehend, more fully than he had done before, how different supplies of blood are sent to particular glands; how various secretions come under the influence of the mind, and how the internal actions of the animal economy, connected with the circulation of the blood, are regulated.

If the healthy actions in the complete animal be thus dependent on nervous influence, then also the restoration of parts injured, the regeneration of parts lost; and all, even the most complicated forms of disease, must be regulated by the natural or preternatural operation of the same machinery.

On the Means of producing a double Distillation by the same heat. By Smithson Tennant, *Esq. F.R.S.* Read June 30, 1814. [*Phil. Trans.* 1814, *p.* 587.]

When steam is passed through a tube surrounded with water, it is well known that it becomes condensed on the sides of the tube so long as the water continues at a lower temperature than that of the steam; but since the latent heat given out in the condensation of steam soon raises the temperature of the water to 212°, all transfer of heat ceases at that temperature, and the steam then passes uncondensed. But since the temperature at which water may be raised into vapour depends on the pressure of the atmosphere, the temperature of the surrounding water may be kept permanently lower, by removing that pressure, so as permanently to act in condensing the vapour of the first distillation; and being itself raised into vapour by mere transfer of the same original quantity of heat, may be received as an additional product of the same process, by a suitable arrangement of the apparatus.

For this purpose, the vessel which serves as condensing worm-tub to the first distillation requires to be fitted with a head and a receiver, all perfectly air-tight; and it may with most convenience be so constructed, that, by application of heat in the first instance to this vessel, the water within may be made to boil completely, so that the air within it will be thereby expelled, and, by a valve or cock, may be prevented from returning when the heat is withdrawn.

An Account of some Experiments on Animal Heat. By John Davy, *M.D. F.R.S.* Read February 17, 1814. [*Phil. Trans.* 1814, *p.* 590.]

The experiments here detailed relate, in the first place, to the relative capacities of venous and arterial blood for heat; secondly, the comparative temperature of these fluids in different parts of the body during life is attempted to be ascertained; and thirdly, the author states those conclusions which he thinks may be drawn from his experiments.

In his first experiments he endeavours to discover the relative capacities by the times of cooling equal volumes of venous and arterial blood, regard being also had to the specific gravities of each. When blood was taken from the jugular vein of a lamb, and after the fibrin had been separated from it by stirring with a wooden spatula, its specific gravity was found to be 1050, that of arterial blood from the same lamb, similarly treated, being 1047. The quantity of each taken for experiment was the same, contained in the same vessel, and heated to the same degree. An equal quantity of water in this vessel had cooled from 120° to 80° in ninety-one minutes; arterial blood cooled, through the same interval, in eighty-nine minutes; and venous blood in eighty-eight minutes: and hence the author infers the capacity of venous blood to be to that of arterial as 92 to 93·7, that of water being 100. By other experiments made on various mixtures of these fluids with each other at different temperatures, he estimates the proportion to be 93 to 93·7.

In subsequent trials on the rates of cooling observed in blood that still contained its fibrin, Dr. Davy estimated the capacities of venous and arterial blood to be as 90 to 91.

The next experiments were upon the proportional heat lost in a given time by mixtures of either kind of blood with water, due allowance being made, as before, for the difference of their specific gravities (viz. 1050 and 1049).

In this mode of trial the proportions were nearly reversed, the capacity of venous blood appearing to be 95·4, whilst that of arterial was no more than 94·8. But the author observes, that these trials admit of less accuracy than the preceding; and he would be inclined to consider the third set of experiments as most entitled to confidence.

Dr. Davy's first experiments on the actual temperatures of venous and arterial blood in the living body, were made at the great vessels of the neck in lambs, sheep, and oxen; and in each a difference was

found from 1° to 1½°, that of arterial blood being in all instances the greater of the two compared.

Since these results are at variance with the observations of Mr. Coleman and Mr. Cooper on the temperature of the two sides of the heart, Dr. Davy also tried the temperatures of these cavities, and found a difference of about 1°; the left ventricle being in all instances warmer than the right.

In the author's experiments on the temperature of different parts of the body, the most remarkable was with respect to that of the brain, which was found to be 1° lower than that of the rectum, although this part was also at least 1° lower than the right side of the heart, and the anterior part of the brain was even 1° or 2° lower than the posterior.

These results, Dr. Davy remarks, are in direct opposition to those of Dr. Crawford in every respect; since he found the capacity of arterial blood greater than that of venous. He found no difference of temperature between the two sides of the heart; and, in fact, the heat of all parts nearly the same. They are, on the contrary, perfectly consistent with Dr. Black's opinion, that animal heat is produced in the lungs; and they are not inconsistent with the hypothesis, that animal heat is dependent on the nervous system.

In conclusion, the author adds some trials that he has made on the temperature of infants just born, which he found to be 2° inferior to that of adults; but on the succeeding day, when respiration was more perfectly performed, he found, on the contrary, an excess of 1°, agreeably to the fourteenth aphorism of Hippocrates.

END OF THE FIRST VOLUME.

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